THURSDAY, JANUARY 11, 1877

FERMENTATION

Études sur la Bière. Par M. L. Pasteur. (Paris: Gauthier-Villars).

I.

IN a recent notice of an article on Brewing contributed by Mr. Pooley to Stanford's "British Manufacturing Industries," some statistics were quoted showing the gigantic development which the production of beer has acquired in the United Kingdom. The amount of beer manufactured in the British Islands, vast though it be, forms yet but a small portion of that made throughout the world. Probably no industry—saving agriculture—employs so large an amount of capital as that of brewing and the various industries supported by it. Important though this old and at present vastly extended industry be, it is yet one which, until ve recently, scientific methods of investigation had done but little to add to our knowledge of the complex phenomena underlying the apparently simple art, and therefore even less in preventing the serious losses constantly occurring even with the most careful manufacturer.

The valuable contribution to our knowledge of the biological aspect of fermentation, which M. Pasteur has just given to the world, is a worthy sequel to the classical researches described by the illustrious chemist in the papers read before the French Academy during the last fifteen years, and summarised by him in a popular form in his "Études sur le Vin," and "Études sur le Vinaigre." These important works, containing the results of long years of laborious and brilliant research, have done much to remove the charge brought against science, and have indeed become to the brewer and wine-grower as the brightening sky to the mariner groping his way in fog and uncertainty to his haven. The grateful recognition of twenty years fruitful labour is due alike from practical and scientific men to the illustrious investigator of the Biology of Fermentation, and he certainly will not deem it the less hearty and sincere if occasional exception here or elsewhere be taken to some of his propositions.

Before considering Fermentation and the recent views promulgated by Pasteur and others regarding the interesting and complex phenomena grouped under this term, it will be useful to discuss the changes produced in the malting and mashing processes of the brewer as necessary precursors to the greater change produced in Fermentation properly so-called. Let us, then, examine the change brought about by the action of water and heat on the grain of any of our common cereals—barley, for example; if this be ground and digested for a few hours at a temperature of 60° C. with five or six times its weight of water, it will be found that the whole, or nearly the whole —this depending upon the comparative activity of the albuminous matters present—of the insoluble starch will be changed into products soluble in water. By the agency of the albuminous ferments or alterative agents water will be assimilated and dextrine and Glucose and products of transformation intermediate between these will be obtained.

This reaction is general, and applies not only to the grain of our common cereals, but also to other stored-up amylaceous vegetable products. While, however, the agency of heat and moisture suffice to bring about this remarkable change, it yet requires too much time, and the products formed are not well adapted to the brewer's wants. Various albumenoid bodies, such as the ptyalin in the saliva, and those in the secretions from the pancreas and intestinal canal in the animal economy intensify the action of the soluble albumenoids in ordinary bread-stuffs, and hence produce a greater hydration, and therefore solution, in a given time. Now the object of malting is to convert some of the inert albumenoids into these more active agents of change, and thus the maltster avails himself of a property existing throughout all vegetable tissues, whereby previously stored-up insoluble amylaceous matter is converted, through the joint agency of moisture, heat, and albumenoid ferments, into soluble sugars, and used along with soluble albumenoid matters for the production of new tissues.

To produce an increase in the albumenoid ferments in barley—the grain chiefly employed in England—the maltster first steeps the grain in water for a period varying from fifty to seventy hours according as the skin is thin or thick, that is, more or less pervious to water. So soon as the desired amount of water has been absorbed, it is removed from the cistern and spread out on the floor of the malt-house, the temperature best suited for the production of sound malt being about 15° C. The germination of the grain commences and is allowed to proceed, with occasional turnings, until the plumule of the young growing plant has advanced far enough up the back of the grain to satisfy the objects which the brewer may have in view. Thus, if a full and somewhat dextrinous ale be required, the plumule is barely allowed to grow to the bend, or *bridge*, of the grain, whereas, if a dry alcoholic ale be required, the growth is allowed to proceed further. According to the growth of the plumule, so does the production of active ferment vary. This point being obtained, the malt is placed on the floor of a kiln, whereby further growth is stopped. Without dwelling with too much detail on this malting, or germinating, process, it will be useful to consider here some of the changes produced. In the first place, by the absorption of water the grain swells and the albumenoid ferments previously existing in the grain are made soluble, and these, by the aid of moisture and heat, begin to act on the starch. Meanwhile, a portion of the insoluble albuminous bodies are rendered soluble, and aid in this conversion of starch, and at the same time, in conjunction with the transformation products of starch, serve to build up the cell structures of the growing plumule and rootlets. Throughout the germinating process carbonic acid and heat are evolved precisely as in the subsequent process of the conversion of sugar into alcohol. The analogy does not, however, rest here, because alcohol is also produced in small quantities. If the growing grain be shut in an air-tight vessel, it will be found that as the growth of the young plant is stopped, the amount of alcohol becomes largely increased, attended at the same time by the production of a large quantity of gas, the chief constituent being carbonic acid. This production of alcohol by vegetable cells has already been noticed in the case of fruits by Lechartier and also by Pasteur. The enormous volume of carbonic acid gas produced without the intervention of *free* oxygen is a fact

of great interest to which we must again refer when considering the action of yeast upon sugar.

In this germinating process we have an example of the way in which insoluble matter stored up, whether in the seed or in the tissues of a plant, becomes digested and used for the production of fresh tissues. This property is not limited to vegetable organisms; in animals, also, we find stored-up matter, fatty or amylaceous, acted upon by albumenoid ferments, and being thus rendered soluble, become available for the building up of fresh structures, or for the production of heat by their oxidation in the system. It would lead us too far to discuss here the manner and the agents by which albuminous food, such as flesh meat, cheese, &c., are made soluble in the human economy. Suffice it here to state that the means by which nature produces the desired end of solution in animal and vegetable alike, are moisture, heat, and albumenoid ferments.

The following analyses by Oudemans show the changes produced in the malting process and in the subsequent drying on the kiln:—

	Barley.	Malt.		
	Air dried.	Air dried.	Kiln dried (pale).	Kiln dried (amber).
Torrefaction products	0·0	0·0	7·8	14·0
Dextrine	5·6	8·0	6·6	10·2
Starch...	67·0	58·1	58·6	47·6
Sugar	0·0	0·5	0·7	0·9
Cellulose	9·6	14·4	10·8	11·5
Albuminous Substances	12·1	13·6	10·4	10·5
Fatty ,, ...	2·6	2·2	2·4	2·6
Ash, &c.	3·1	3·2	2·7	2·7
Total	100·0	100·0	100·0	100·0

These analyses, though made some years since, and differing in some points from our existing knowledge, are yet sufficient for our present purpose. We see that the air-dried malt, when compared with the barley from which it was made, is poorer in starch, and richer in woody fibre.

Again, as regards the changes in the albuminous bodies, still quoting Oudemans, we find—

	Barley.	Malt.
Gluten, soluble in alcohol	0·28	0·34
Albuminous substances coagulable by heat	0·28	0·45
,, ,, not coagulable ...	1·55	2·08
Insoluble albumen	7·59	6·23
Total	9·70	9·10

We notice that there has been a loss of some of the albuminous matters in the germinating process, but at the same time an increase in the soluble or active agents of change, to obtain which the malting process is followed. Let us now examine the changes which the malt undergoes when placed in the brewer's mash tun and submitted to the action of heat and water. In England the amount of water employed varies with the nature of the beer to be made, it may be taken, however, as being about twice the weight of the malt, and the temperature of the mixture of malt and water varies from 63° C. to 67° C. In the course of half-an-hour the insoluble starch is converted into soluble sugars and dextrine. The infusion thus obtained is, however, too dextrinous in character for the production of dry alcoholic ales, but as the process continues, more and more water is assimilated, and finally an infusion is obtained rich in alcohol-yielding sugars.

Many theoretical explanations have been given, and will doubtless yet be given, as to the action and the products formed. We are indebted to the admirable and suggestive researches of O'Sullivan for the first clear exposition of what takes place, or rather of one of the changes which occur. He formulates the reaction thus:—

$$\underset{\text{Starch}}{C_{18}H_{30}O_{15}} + H_2O = \underset{\text{Dextrine}}{C_6H_{10}O_5} + \underset{\text{Maltose}}{C_{12}H_{22}O_{11}},$$

and he states that this is the final action of soluble ferments on starch. This equation, however, expresses only what takes place when a small quantity of malt acts on starch paste at a temperature of only 40° C. to 45° C. We must rather assume the probability of several changes, of which that formulated by O'Sullivan is one. Thus if the amount of starch be large in comparison to the active albumenoid ferments, and if the water and time of infusion be lessened, we obtain a solution having a reducing power on Fehling's liquid equal to 33 per cent. of glucose. On the other hand, if the conditions be reversed, that is, if we increase the time, the amount of ferment, and the water, we obtain an infusion having a reducing power equal to 66 per cent. of glucose.

Here, however, the action ceases, and the so-called diastase of malt is unable to carry on the hydration beyond the point last-mentioned, where we obtain a solution which reduces Fehling's liquid to the extent of two-thirds of the reduction obtained when the starch is fully hydrated by weak mineral acids.

Formulating the reactions according to the Fehling reducing products obtained we should have—

$$\left\{\begin{array}{l}\underline{C_{12}H_{20}O_{10} + 2H_2O}\\ C_{12}H_{20}O_{10}\\ C_{12}H_{20}O_{10};\end{array}\right.$$

this product having a 33·33 per cent. reducing action. On the other hand, where we continue the action of diastase for a longer period and with more water, we have—

$$\left\{\begin{array}{l}\underline{C_{12}H_{20}O_{10} + 2H_2O}\\ \underline{C_{12}H_{20}O_{10} + 2H_2O}\\ C_{12}H_{20}O_{10};\end{array}\right.$$

this product having a 66·66 per cent. reducing power, and being apparently the final action of diastase on starch. When the hydration of the starch is effected by dilute acid we obtain—

$$\left\{\begin{array}{l}\underline{C_{12}H_{20}O_{10} + 2H_2O}\\ \underline{C_{12}H_{20}O_{10} + 2H_2O}\\ C_{12}H_{20}O_{10} + 2H_2O;\end{array}\right.$$

that is, dextrose having the full reducing action.

O'Sullivan's dextrine-maltose reaction might similarly be thus expressed—

$$\left\{\begin{array}{l}\underline{C_{12}H_{20}O_{10} + H_2O}\\ \underline{C_{12}H_{20}O_{10} + H_2O}\\ C_{12}H_{20}O_{10}\end{array}\right.$$

and giving a product having a 44·44 per cent. reducing

action. If this be a correct expression it follows that there are two other possible cases where one and three molecules of water are assimilated. Cane-sugar and lactose are isomeric with O'Sullivan's maltose, and are all dextro-rotatory. These three sugars of the formula $C_{12}H_{22}O_{11}$ are all further hydrated by the action of diastase, yeast water, and dilute acids.

Though ignorant of the molecular weights of starch we may yet safely assume in the expression $(C_6H_{10}O_5)_n$ that the value of *n* must be large, and that a molecule of such complexity will yield in addition to dextrine several sugars isomeric with sucrose and with dextrose, and it may be others of greater hydration. In the brewer's mash tun the hydration products vary according to the malt employed and the extent to which the malting process is pushed before the kiln drying action, the more of the active ferment formed the greater the hydration in a given time. The amount of water is an important factor in the hydration process; the greater this is within certain limits the greater the hydration; and lastly, the products vary with the amount of time given to the mashing or hydration fermentation.

Thus we may briefly sum up the changes produced by the statement that the infusion products of starch will be more dextrinous, *i.e.*, least altered according as we lessen the time, the amount of water, and the growth of the plumules in the malting process; and on the other hand the infusion products will be richer in Glucose (dextrose), and therefore attenuate lower in the subsequent fermentation process, according as we increase the amount of water, the time of infusion, and the growth of the plumule. The variations in these directions introduced in the mashing process in English breweries are within a narrow range, and the products formed have a reducing action on Fehling's liquid, varying from 50 per cent. to 55 per cent. of the total hydration possible by the aid of mineral acids.

The use of unmalted grain is prohibited in England, whereas cane-sugar and Glucose (made by the action of dilute acid on grain) are allowed. The variations in the direction of dextrine-increase were until recently very limited, but on the other hand those in the direction of alcohol-yielding sugars are without limit.

Messrs. O'Sullivan and Valentin, in a communication to the Society of Arts (March 17, 1876), have recently shown how the action of dilute sulphuric acid may be so regulated as to obtain O'Sullivan's dextrine-maltose reaction already described.

The hydration by the agency of very dilute sulphuric acid is carried on until the liquid has a rotatory power of $+171^\circ$, indicating two parts of maltose (rotatory power $+150^\circ$) and one part of dextrine (rotatory power according to O'Sullivan $+213^\circ$), *i.e.*,

$$\frac{2 \times 150 + 213}{3} = +171^\circ.$$

So soon as the polariscope indicates O'Sullivan's reaction to be complete, the further hydration is stopped by the addition of chalk. Should the mixture of dextrine and maltose thus made prove to yield a stable and good-keeping beer, they will have contributed greatly to counteract the evil tendency of recent legislation by which beer more and more alcoholic has been manufactured.

Having briefly examined the hydration of starch by albumenoid alterative ferments in the brewer's mash-tun, we have now to consider the breaking up of the still complex saccharine products of the reaction into bodies of simpler structure, such as alcohol and carbonic acid, which result from the fermentation process properly so called. Though it is with alcoholic fermentation, with its characteristic boiling or disengagement of carbonic acid gas, that we have chiefly to do, at the same time other products of the decomposition of saccharine bodies, such as acetic, lactic, and butyric acids, must necessarily be considered before we can obtain a correct insight into the phenomena which present themselves in the manufacture of beer.

Let us then follow the products formed by the hydration of starch already studied.

The *wort*, as the brewer terms the liquid containing the infusion products of the mash tun, is drawn off from the insoluble matters of the malt, and is then boiled in another vessel along with hops; the amount of this valuable agent of preservation employed depending upon the strength of the wort, the nature of the product desired, and the length of time it has to be kept before being consumed.

By mere boiling, some of the albuminous bodies are rendered insoluble, a further portion is precipitated by the tannin of the hops, and the resulting liquid, being thus deprived of some of the albuminous food materials, is found to be less liable to subsequent destructive changes. The hops at the same time yield a pleasant bitter principle, and essential oils which play no slight part in the preservation of the manufactured beer. Now, unlike the juice of the grape, the infusion of malt is so rich in albuminous matters, that every expedient is adopted to diminish these aids to destruction; hence the process of boiling, the use of tannin, and the employment in the infusion process of hard water containing salts of lime. To its water Burton chiefly owes its reputation for good ale. The boiled wort, when cooled, is placed in fermenting vessels, and *yeast* is added. This addition of yeast is almost universal; at the same time it must be noted that in the production of *Faro* and *Lambick* the Belgian brewer adds no ferment; a similar practice was at one time rather common in England, and is even now occasionally to be found in Wiltshire. In thus adding no ferment, the brewer follows the invariable practice of the wine-maker, who leaves the must or pressed juice to spontaneous fermentation; the wine-grower may reasonably reckon upon a definite decomposition of his must, but the brewer who follows this method can foretell but little of the result. We shall presently see why the wine-grower's must and the brewer's wort comport themselves so differently under apparently the same conditions. The spontaneous fermentation of malt wort, even now so little practised, is doomed to be altogether discontinued within but a few years.

The English brewer, having cooled his wort to a temperature varying from 14° C. to 18° C., and having added yeast, the fermentation commences, the heat, unless checked, rapidly rises, and the yeast greatly increases in quantity, the larger portion of which rises to the surface of the liquid. Hence this is termed *top* or surface fermentation, in order to distinguish it from the Bavarian process, in which the yeast sinks to the bottom of the

liquid. The temperature of the German *bottom* fermentation varies from $5^{\circ}\cdot5$ C. to 7° C., a temperature that can only be maintained by the employment of large quantities of ice.

The *bottom* and *top* yeasts are probably distinct species. M. Pasteur, however, seems to be in error in stating (p. 190) that the bottom yeast may be distinguished by being less spherical than top yeast. It is true that in London and Edinburgh yeast the cells will be found usually round; hard water, however, such as that at Burton, or artificially made so, yields yeast in which the cells are distinctly ovoid in appearance, resembling very closely Bavarian bottom yeast. M. Pasteur further states (pp. 188 and 192) that the bottom yeast yields a beer of finer flavour, and hence argues the replacement of ales produced by top fermentation by those made on the Bavarian system. Here surely he must be thinking rather of the inferior products of the surface fermentation in France and Germany than of those of England and Scotland. His assertions (pp. 12–17) that by bottom fermentation store beers can be produced, whereas those produced by top fermentation must be consumed at once and cannot be transported are certainly strange to an Englishman.

So far from these unfavourable comparisons being true in all cases, the exact opposite is generally the case. Bavarian and other bottom fermentation beers are in fact those which can neither be preserved nor transported without the liberal employment of ice; even that sent from Vienna to London must be kept cold artificially in order to avoid rapid destruction. As regards flavour, there are many who think a glass of Burton pale ale or of good old college rent ale to be superior to any Bavarian beer. The chief cause of the decline in the production of top fermentation beers on the Continent has been the want of attention in the fermentation process, whereas the English brewer, especially the brewer of high-class ales, has been unremitting in his attention to the temperature in fermentation and to the perfect cleansing of the ale. Now where such attention is given it is not difficult to obtain ales which will keep a few years. While objecting to our English produce being so hastily depreciated by M. Pasteur, our brewers will be the first to avail themselves of his biological researches in order to render their produce more stable and better flavoured, without having recourse to the general adoption of the vastly more costly system of bottom fermentation.

Let us now leave this question of the respective value and future development of the two systems of fermentation, and assume that by either the one process or the other we have obtained our glass of beer. The question now naturally presents itself to us, as to others before us, to what is fermentation due? Pasteur's answer to this I propose to discuss next week.

CHARLES GRAHAM

OUR BOOK SHELF

Manual of the Vertebrates of the Northern United States. By David S. Jordan, M.D. (Chicago: Jansen, McClurg, and Co., 1876.)

THIS useful work contains a short diagnostic account of the whole of the vertebrated animals of the Northern United States, and has been written, as the author tells us, to give collectors and students who are not specialists a ready means of identifying the families, genera, and species described. The mammals as well as the birds of North America have been so ably and elaborately treated of by Prof. Baird, Dr. Coues, and others, that those who are studying these branches of zoology will not find this smaller volume of special service, nevertheless we are not acquainted with any work having a range of treatment which includes the reptilia, amphibia, and fishes with the two other classes. The sub-kingdom, as well as each class and order, are concisely defined, and the most modern arrangement is adopted, based upon the best authorities, the relative importance of the characterising features being clearly brought forward. The system of employing artificial keys so useful in botanical determinations, and so successfully employed by Dr. Coues in ornithology, is employed throughout the book, and will, no doubt, be found to work well. A glossary of the principal technical terms used in the body of the book is also appended. As an example of the manner in which the different species are described, we will take that of one of the species of Fly-catchers: "*Empidonax acadicus* (Gm.), Baird. SMALL GREEN-CRESTED FLY-CATCHER.—Clear olive-green; wing bands buffy; whitish becoming yellowish below; yellowish ring about eyes; bill pale below; primaries nearly an inch longer than secondaries; second, third, and fourth primaries nearly equal, and much longer than first and fifth; first much longer than sixth; L. 6; W. 3; T. $2\frac{3}{4}$; Ts. $\frac{2}{3}$; Tcl. $\frac{1}{2}$; E.U.S. frequent." To naturalists on this side the Atlantic the work will be found a valuable one of reference on account of its inclusiveness, and a glance through it makes us feel how useful a similar one on the British vertebrate fauna would prove to students and collectors.

The Emigrant and Sportsman in Canada. By John J. Rowan. (London: Stanford, 1876.)

THIS is a capital book in many respects. Mr. Rowan is himself an old Canadian settler and knows the country well in various aspects. He tells the plain truth as to the suitability of Canada as a field for emigration, and the intending emigrant could not get a better guide as to the resources of the country, and the kind of settlers for which it is adapted. Mr. Rowan is a keen sportsman and has a fair knowledge of zoology. His descriptions of hunting life in Canada are thoroughly interesting and abound with fresh information on the many animals which are still to be found there. Mr. Rowan is a good observer, and some of the information which he gives regarding the animals with whose habits he is familiar may be new even to naturalists. He describes, at considerable length, especially, the habits of the beaver as observed by himself, and adduces some facts to show that previous popular statements with regard to this animal must be to some extent modified. The volume will be found of interest not only to the emigrant, the sportsman, and the naturalist, but to all who love good hunting and trapping stories well told. Its principal defect is the want of an index.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

On a Mode of Investigating Storms and Cyclones

I SCARCELY know anything more interesting in connection with the investigation of cyclones and of our storms than the theoretical investigations of Reye, Mohn and Guldberg, and the practical ones of Mr. Clement Ley. Mr. Ley's papers in the *Journal* of the Scottish Meteorological Society, iv. 66, 149, 330, have especially attracted my attention. We have to study the

form of the craters, as I might call them, of the barometric depressions, and their steepness in different directions.

The following note has some connection with this inquiry, and I beg you, if you think it suitable, to give it a place in your esteemed journal :—

In 1874 I proposed to lay, as well as it can be done, a plane or planes, having such a slope as would represent the barometric height at some two distant places, and to indicate (in geodetic terms) the fall and strike, or the inclination on the horizon and the azimuth of the projection of the perpendicular on such a plane, and I still recommend it. In the Netherlands, where my area is small (see Jour. Scot. Met. Soc., iv. 25) it is always easy to find such a plane, and of course its perpendicular. Now I have inquired whether the projection of that perpendicular moved round the horizon generally in a direct way (with the sun) in the same manner as M. Dove has found that the direction of the wind does, and which I demonstrated in *Pogg. Ann.*, lxviii. 417, 553, to be the case thirteen times per annum in our latitude.

On examination I find that in 1874 and 1875 the projection has gone round the horizon in a direct way ten times more than the opposite way ; further, that it often goes back when the direction of the projection lies to the south-east, but that when it has veered to be to the north-west it veers forward surely and quickly enough in a direct way to the east, which is in accordance with the fact that when we have a depression over Ireland or Scotland it then moves in the direction of Norway and Finland. I don't think it superfluous to call the attention of others to this research, and I propose to calculate the results for other years in this respect, which is easily done by means of the Netherlands' *Annuaires*, and thus find thrice a day the direction and size of the steepest gradient.

Utrecht, December 23, 1876 BUYS BALLOT

Mind and Matter

THE problem, "How consciousness stands related to the material organism," has been attempted to be solved by Mr. Duncan, under the head of "Mind and Matter" (NATURE, vol. xv., p. 78). Now that a more exact scientific examination has reconciled so many differences on this question, a return to the old *à priori* method of mere logic is still perfectly legitimate, provided the logic is sound.

Admitting that consciousness is *related* to matter, and without contending, for the present, that it may not be a *state* of matter (under certain restrictions of the term), I will content myself with pointing out what seem to be fallacies in this "solution." "It is as easy," says Mr. Duncan, "to predicate subjectivity (susceptibility to consciousness) of one entity called matter as of another entity called soul or spirit. It is no more difficult to conceive of matter being subjective than of spirit being subjective." Let us see if this is or is not petitio principii. It was the difficulty, real or apparent, of ascribing certain attributes (mental) to matter, that demanded the supposition of some support other than material. So that when we say that spirit is alone susceptible to consciousness, we merely express that matter is not thus susceptible. Therefore, to affirm that the one may be as susceptible to consciousness as the other is to assume, *in limine*, that matter may be susceptible to consciousness, the very probability which has to be established.

Mr. Duncan next asserts that "How energy is related to matter in all its forms, is no less mysterious than how subjectivity may be a property of matter." Now every opponent of materialism admits that how energy is *related* to matter is a mystery, and avows that he cannot conceive of consciousness as a *property* of matter ; but the difficulty of understanding the *how*, even if we grant it equal in both cases, cannot establish any parity of probability as to the facts ; for while we know as a fact that energy is *related* to matter, we do not know as a fact that subjectivity (susceptibility to consciousness) is a *property* of matter. And even if we put the argument more exactly, and affirm that we know that subjectivity, like energy, is *related* to matter, still nothing in point is gained, seeing that while we know *all* matter in relation to energy, it is only a certain form of matter (the human) which we *know* to be related to subjectivity ; for if we surmise this of a dog, we cannot *know* it till he tell us.

The next position, "Energy may be divided. Why not subjectivity?" would seem to demand nothing less than absolute proof, since subjectivity, or the state of the Ego, appears indivisible in virtue of its essential unity. Yet no support is advanced except the foregoing assertion, which we have seen is a mere assumption on the side of materialism, and which we shall next see contains an admission all but fatal to the cause it advocates. When Mr. Duncan says, "How energy is related to matter is no less mysterious than how subjectivity may be a property of matter," he admits that we cannot understand either, while he believes the first because it is a fact. But why should we believe the last? Because we cannot understand it, and because it is not a fact ? Will he admit that we have advanced any proof of an oyster being an astronomer, when we have affirmed that this would be no more mysterious than the relation of energy to matter? Yet his three remaining arguments go on this ground : they assume that the probability of subjectivity being a property of matter equals the fact of energy being related to matter.

Rugby J. L. TUPPER

Solar Physics at the Present Time

AT the conclusion of his letter of the 1st inst. (NATURE, vol. xv. p. 196), Sir G. B. Airy alludes to a paper of mine as being cited by me (in my last letter to NATURE) as being "in the 'Philosophical Transactions.'"

The paper referred to *ought*, with little doubt, to have appeared there, but it did not, and I was most careful to avoid implying that it had ; my words being with regard to it (see your pages 157 and 158) :—

1st, "which I had the honour of communicating to the Royal Society of London *six* years ago ;" and

2nd, "that paper of six years ago, and still in the hands of the Royal Society ;"

nor is there any mention of the "Philosophical Transactions" throughout. PIAZZI SMYTH,

Edinburgh, January 5 Astronomer Royal for Scotland

Towering of Birds

SNIPE frequently tower—also pigeons. I saw a mallard that flew nearly half a mile, towered, and, fell dead. Teal also tower, but their towering is different to the ordinary, as they are as often alive as dead when they fall. I have also remarked this in widgeons, and once in a partridge. In the latter case birds fell right and left, the second a towerer. It was in heavy turnips that had been planted when mangel had missed. The towerer fell on an isolated mangel ; when picked up, he was at least ten yards from the mangel and still alive. Some years ago there was a discussion on this subject in *Land and Water* or the *Field*, and I think it was shown it was due to pulmonary hæmorrhage. At least I was quite aware of the cause, and that head or spine injuries had nothing to do with it.

Ovoca, Ireland G. H. KINAHAN

Rooks Building at Christmas

ON Christmas morning I saw a few rooks engaged in building in a clump of elms near my house. Four nests are now in progress, though the gale of December 30 made the rooks desist from their work. During the ten years (about) that I have watched their proceedings, I think I have never seen these birds begin building till February.

I may add that our well-watered lands and woods are being visited with wild duck, teal, peewits, and gulls in great numbers. C. M. INGLEBY

Valentines, Ilford, Essex

Are We Drying Up?

THE above question has been asked in the columns of NATURE. As a small contribution towards an answer, it may be stated that at this place the two last years, 1875 and 1876, have been the two wettest in a series of twenty-four years.

In 1875, the rainfall was 44·05 inches.

In 1876 ,, ,, 42·42 ,,

The average of twenty-four years has been 33·11 inches.

Clifton, January 7 GEORGE F. BURDER

Radiant Points of Shooting Stars

IN December, from observations of 163 shooting stars seen in 20½ hours' watching, chiefly in the evenings, I amply confirmed several of the positions of radiant points as given in my note (NATURE, vol. xv., p. 158), and observed that several of the showers there mentioned were actively continued. The centres, as I gave them, of two of these require revision, as the additional meteors seen in December indicate the radiants with

greater precision than the few I had noted in November. The new radiant in Sextans, I now deduce at R.A. 148°, Decl. 2° N., and that at τ Leonis, as near Crater R.A. 165°, Decl. 6° S. The meteors from these new showers are very rapid and white, usually leaving bright streaks for 2 or 3 secs. in their path.

Ashley Down, Bristol, January 2 W. F. DENNING

ALEXANDER BAIN

IT is with much regret that we announce the death of Mr. Alexander Bain, which took place at Glasgow on January 2. To many of our readers his name is perhaps unknown, and yet the inventions of Mr. Bain, made when telegraphy was in its infancy, were of the very highest importance. They were perhaps made too soon. Mr. Bain himself never reaped the benefit of them, and would have died in great poverty had it not been for a pension of 80*l.* a year obtained for him from Mr. Gladstone chiefly through the exertions of Mr. C. W. Siemens, Sir William Thomson, and the Society of Telegraph Engineers.

One of the most important services of Mr. Bain to telegraphy was the reinvention of the method of making use of "bodies of natural waters" "to complete the electric circuit by laying a single insulated wire between the given stations, having at each end a metallic brush immersed in the water." In 1838 Steinheil discovered the use of the earth for completing a circuit instead of a return wire, but does not appear to have taken steps to bring his discovery into notice, or to remove the prejudices with which a discovery so startling would naturally be met. Mr. Bain seems to have established the principle for himself, and he published it in a patent of 1841, by Wright and Bain, for "Improvements in applying electricity to control railway engines and carriages, to mark time, to give signals, and to print intelligence at different places." It is impossible to say how large a part of the completeness of our present telegraphic system, particularly of our submarine telegraphic system, is due to this great discovery of Steinheil and Bain.

An early invention by Mr. Bain, was that of the electro-chemical telegraph. This was patented in 1846. Paper chemically prepared is drawn under a metallic style which rubs upon it. As long as there is no current passing in the line the paper comes away from the style unmarked, but each signal sent through the line passes by the style to the prepared paper and leaves a mark. Combinations of dots and dashes, as in the Morse system, formed Mr. Bain's alphabet.

At first the signals were sent by hand by a simple contact key, but Mr. Bain soon found his system capable of receiving signals at far higher speed than that of the fastest hand sending. He was thus led to the invention of automatic methods of transmitting signals of which one is the basis of the most important method at present in use. A slip of paper is perforated with holes arranged in groups, forming the letters required in accordance with the code of signals. This slip is passed between a metallic roller and a contact point. As long as the contact point is separated from the roller by the paper slip, no current passes in the line. But when one of the perforated holes comes under the contact point, the point falls in and makes contact with the metallic roller. The circuit is thus closed, and a signal is sent.

This apparatus was tried before Committees of the Institute and of the Legislative Assembly at Paris. Through a line between Paris and Lille, a message of 282 words was sent. The time taken was fifty-two seconds! The fastest automatic receiving by mechanical instruments of the most refined modern construction, such, for example, as the instruments of Wheatstone, does not commonly reach 100 words per minute. We hear from Sir William Thomson, in his recent address to the British Association, that he *saw* in America "Edison's Automatic Telegraph delivering 1,057 words in 57 seconds—this done by the electro-chemical method of Bain." That Mr. Bain's method was not received in England cannot but be regarded as a great misfortune.

These were, perhaps, Mr. Bain's principal inventions, but there are others of such importance that they well deserve notice. Several of his patents relate to the keeping of time by clocks controlled or driven electrically by a standard clock. Jones' system, now so largely used in England, is based upon the system of Bain. He invented the earth battery in 1843, or rather reinvented it, as Gauss and Steinheil had previously obtained a current, after the discovery by Steinheil of using the earth for a return wire, making one of the earth plates of zinc and the other of copper. In 1844 he patented ingenious apparatus for registering the progress of ships and for taking soundings. Vanes caused to rotate by the motion of the "log" or "sounding fly," through the water were employed, and an electrical method of observing the result on board was employed. The same patent describes apparatus for giving warning when the temperature of the hold of a ship rises above a certain point. An electric circuit was employed, which was closed by the expansion by heat of mercury contained in a tube. The current passing in the circuit traversed coils which formed an electro-magnet. A pointer or alarm connected with the magnet gave the required warning. This method is now very commonly employed for fire alarms; and modifications of it have been proposed for giving warning of over-heating in the bearings of machinery.

He had also an electric method of playing a keyed instrument at a distance on more than one organ or piano at a time; and he applied his perforated paper to the automatic playing of a wind instrument, such as an organ. For this purpose the paper, properly punched, was drawn between the openings of the wind chest and the openings of the notes to be played upon. Whenever and as long as there was a punched hole of the paper between the wind chest and the pipe the note of the pipe sounded. When there was a blank space between the wind chest and pipe the pipe was silent.

In his later years Mr. Bain's inventions have been inconsiderable. Some years ago he was stricken down with paralysis. He died at the age of sixty-six, on the second day of this year, in the Home for Incurables, Broomhill, near Glasgow.

PHOTOGRAPHS OF THE SPECTRA OF VENUS AND α LYRÆ

SINCE the spring of 1872 I have been making photographs of the spectra of the stars, planets, and moon, and particularly among the stars, of α Lyræ and α Aquilæ, with my 28-inch reflector and 12-inch refractor. In the photograph of α Lyræ, bands or broad lines are visible in the violet and ultra-violet region, unlike anything in the solar spectrum. The research is difficult, and consumes time, because long exposures are necessary to impress the sensitive plate, and the atmosphere is rarely in the best condition. The image of a star or planet must be kept motionless for from ten to twenty minutes, and hence the driving-clock of the telescope is severely taxed.

During last summer I obtained some good results, and in October took photographs of the spectrum of Venus, which show a large number of lines. I am now studying these pictures, and have submitted them to the inspection of several of my scientific friends, among others, Professors Barker Langley, Morton, and Silliman. There seems to be in the case of Venus a weakening of the spectrum towards H and above that line of the same character as that I have photographically observed to take place in the spectrum of the sun near sunset.

New York, December, 1876 HENRY DRAPER

ON THE STUDY OF BIOLOGY[1]

IT is my duty to-night to speak about the study of Biology, and while it may be that there are many among you who are quite familiar with that study, yet as a lecturer of some standing, it would, I know by experience, be very bad policy on my part to suppose such to be extensively the case. On the contrary, I must imagine that there are many of you who would like to know what Biology is; that there will be others who have that amount of information, but would nevertheless gladly learn why it should be worth their while to study Biology; and yet others, again, to whom these two points are clear, but who desire to learn how they had best study it, and finally when they had best study it; and I shall address myself to the endeavour to give you some answer to these four questions—what Biology is, why it should be studied, how it should be studied, and when it should be studied.

In the first place, in respect to what Biology is, there are, I believe, some persons who imagine that the term "Biology" is simply a new-fangled denomination, a neologism in short, for what used to be known under the title of "Natural History," but I shall try to show you, on the contrary, that the word is the expression of the growth of science during the last 200 years, and came into existence half a century ago.

At the revival of learning, knowledge was divided into two kinds—the knowledge of nature and the knowledge of man; for it was the current idea then (and a great deal of that ancient conception still remains) that there was a sort of essential antithesis, not to say antagonism, between nature and man; and that the two had not very much to do with one another, except that the one was oftentimes exceedingly troublesome to the other. Though it is one of the salient merits of our great philosophers of the seventeenth century, that they recognise but one scientific method, applicable alike to man and to nature, we find this notion of the existence of a broad distinction between nature and man in the writings of Bacon and Hobbes of Malmesbury; and I have brought with me that famous work which is now so little known, greatly as it deserves to be studied, "The Leviathan," in order that I may put to you in the wonderfully terse and clear language of Thomas Hobbes, what was his view of the matter. He says:—

"The register of knowledge of fact is called history. Whereof there be two sorts, one called natural history; which is the history of such facts or effects of nature as have no dependence on man's will; such as are the histories of metals, plants, animals, regions, and the like. The other is civil history; which is the history of the voluntary actions of men in commonwealths."

So that all history of fact was divided into these two great groups of natural and of civil history. The Royal Society was in course of foundation about the time that Hobbes was writing this book, which was published in 1651, and that Society is termed a "Society for the Advancement of Natural Knowledge," which is nearly the same thing as a "Society for the Advancement of Natural History." As time went on, and the various branches of human knowledge became more distinctly developed and separated from one another, it was found that some were much more susceptible of precise mathematical treatment than others. The publication of the "Principia" of Newton, which probably gave a greater stimulus to physical science than any work ever published before, or which is likely to be published hereafter, showed that precise mathematical methods were applicable to those branches of science such as astronomy, and what we now call physics, which occupy a very large portion of the domain of what the older writers understood by natural history. And inasmuch as the partly deductive and partly experimental methods of treatment to which Newton and others subjected these branches of human knowledge, showed that the phenomena of nature which belonged to them were susceptible of explanation, and thereby came within the reach of what was called "philosophy" in those days; so much of this kind of knowledge as was not included under astronomy came to be spoken of as "natural philosophy"—a term which Bacon had employed in a much wider sense. Time went on, and yet other branches of science developed themselves. Chemistry took a definite shape, and as all these sciences, such as astronomy, natural philosophy, and chemistry, were susceptible either of mathematical treatment or of experimental treatment, or of both, a great distinction was drawn between the experimental branches of what had previously been called natural history and the observational branches—those in which experiment was (or appeared to be) of doubtful use, and where, at that time, mathematical methods were inapplicable. Under these circumstances the old name of "Natural History" stuck by the residuum, by those phenomena which were not, at that time, susceptible of mathematical or experimental treatment; that is to say, those phenomena of nature which come now under the general heads of physical geography, geology, mineralogy, the history of plants, and the history of animals. It was in this sense that the term was understood by the great writers of the middle of the last century—Buffon and Linnæus—by Buffon in his great work, the "Histoire Naturelle Générale," and by Linnæus in his splendid achievement, the "Systema Naturæ." The subjects they deal with are spoken of as "Natural History," and they called themselves and were called "Naturalists." But you will observe that this was not the original meaning of these terms; but that they had, by this time, acquired a signification widely different from that which they possessed primitively.

The sense in which "Natural History" was used at the time I am now speaking of has, to a certain extent, endured to the present day. There are now in existence, in some of our northern universities, chairs of "Civil and Natural History," in which "Natural History" is used to indicate exactly what Hobbes and Bacon meant by that term. There are others in which the unhappy incumbent of the chair of Natural History is, or was, still supposed to cover the whole ground of geology and mineralogy, zoology, perhaps even botany in his lectures. But as science made the marvellous progress which it did make at the latter end of the last and the beginning of the present century, thinking men began to discern that under this title of "Natural History" there were included very heterogeneous constituents—that, for example, geology and mineralogy were, in many respects, widely different from botany and zoology; that a man might obtain an extensive knowledge of the structure and functions of plants and animals without having need to enter upon the study of geology and mineralogy, and *vice versâ*; and, further, as knowledge advanced, it became clear that there was a great analogy, a very close alliance, between those two sciences of botany and zoology which deal with living beings, while they are much more widely separated from all other studies. It is due to Buffon to remark that he clearly recognised this great fact. He says: "ces deux genres d'êtres organisés [les animaux et les végétaux] ont beaucoup plus de propriétés communes que de différences réelles." Therefore, it is not wonderful that, at the beginning of the present century, and oddly enough in two different countries, and so far as I know, without any intercommunication, two famous men clearly conceived the notion of uniting the sciences which deal with living matter into one whole, and of dealing with them as one discipline. In fact I may say there were three men to whom this idea occurred contemporaneously, although there were but two who carried it into effect, and only one who worked it out completely. The persons to

[1] A lecture by Prof. Huxley, delivered at the South Kensington Museum on Saturday, December 16, 1876.

whom I refer were the eminent physiologist Bichat, the great naturalist Lamarck, in France; and a distinguished German, Treviranus. Bichat[1] assumed the existence of a special group of "physiological" sciences. Lamarck, in a work published in 1801,[2] for the first time made use of the name "Biologie" from the two Greek words which signify a discourse upon life and living things. About the same time, it occurred to Treviranus that all those sciences which deal with living matter are essentially and fundamentally one, and ought to be treated as a whole, and, in the year 1802, he published the first volume of what he also called "Biologie." Treviranus's great merit consists in this, that he worked out his idea, and wrote the very remarkable book to which I refer. It consists of six volumes, and occupied its author for twenty years—from 1802 to 1822.

That is the origin of the term "Biology," and that is how it has come about that all clear thinkers and lovers of consistent nomenclature have substituted for the old confusing name of "Natural History," which has conveyed so many meanings, the term "Biology" which denotes the whole of the sciences which deal with living things, whether they be animals or whether they be plants. Some little time ago—in the course of this year, I think—I was favoured by a learned classic, Dr. Field of Norwich, with a disquisition, in which he endeavoured to prove that, from a philological point of view, neither Treviranus nor Lamarck had any right to coin this new word "Biology" for their purpose; that, in fact, the Greek word "Bios" had relation only to human life and human affairs, and that a different word was employed when they wished to speak of the life of animals and plants. So Dr. Field tells us we are all wrong in using the term biology, and that we ought to employ another, only unluckily he is not quite sure about the propriety of that which he proposes as a substitute. It is a somewhat hard one—zootocology. I am sorry we are wrong, because we are likely to continue so. In these matters we must have some sort of "Statute of Limitations." When a name has been employed for half-a-century, persons of authority[3] have been using it, and its sense has become well understood, I am afraid that people will go on using it, whatever the weight of philological objection.

Now that we have arrived at the origin of this word "Biology," the next point to consider is: What ground does it cover? I have said that in its strict technical sense it covers all the phenomena that are exhibited by living things, as distinguished from those which are not living; but while that is all very well so long as we confine ourselves to the lower animals and to plants, it lands us in a very considerable difficulty when we reach the higher forms of living things. For whatever view we may entertain about the nature of man, one thing is perfectly certain, that he is a living creature. Hence, if our definition is to be interpreted strictly, we must include man and all his ways and works under the head of Biology; in which case we should find that psychology, politics, and political economy, would be absorbed into the province of Biology. In fact, civil history would be merged in natural history. In strict logic it may be hard to object to this course, because no one can doubt that the rudiments and outlines of our own mental phenomena are traceable among the lower animals. They have their economy and their polity, and if, as is always admitted, the polity of bees and the commonwealth of wolves fall within the purview of the biologist proper, it becomes hard to say why we should not include therein human affairs, which in so many cases resemble those of the bees in zealous getting, and are not without a certain parity in the proceedings of the wolves. The real fact is that we biologists are a self-sacrificing people; and inasmuch as, on a moderate estimate, there are about a quarter of a million different species of animals and plants to know about already, we feel that we have more than sufficient territory. There has been a sort of practical convention by which we give up to a different branch of science what Bacon and Hobbes would have called "Civil History." That branch of science has constituted itself under the head of Sociology. I may use phraseology which at present will be well understood and say that we have allowed that province of Biology to become autonomous; but I should like you to recollect that that is a sacrifice, and that you should not be surprised if it occasionally happens that you see a biologist trespassing upon questions of philosophy or politics; or meddling with human education; because, after all, that is a part of his kingdom which he has only voluntarily forsaken.

Having now defined the meaning of the word Biology, and having indicated the general scope of Biological Science, I turn to my second question, which is—Why should we study Biology? Possibly the time may come when that will seem a very odd question. That we, living creatures, should not feel a certain amount of interest in what it is that constitutes our life will eventually, under altered ideas of the fittest objects of human inquiry, seem to be a singular phenomenon; but at present, judging by the practice of teachers and educators, this would seem to be a matter that does not concern us at all. I propose to put before you a few considerations which I dare say many of you will be familiar with already, but which will suffice to show—not fully, because to demonstrate this point fully would take a great many lectures—that there are some very good and substantial reasons why it may be advisable that we should know something about this branch of human learning. I myself entirely agree with another sentiment of the philosopher of Malmesbury, "that the scope of all speculation is the performance of some action or thing to be done," and I have not any very great respect for, or interest in, mere knowing as such. I judge of the value of human pursuits by their bearing upon human interests; in other words, by their utility, but I should like that we should quite clearly understand what it is that we mean by this word "utility." Now in an Englishman's mouth it generally means that by which we get pudding or praise, or both. I have no doubt that is one meaning of the word utility, but it by no means includes all I mean by utility. I think that knowledge of every kind is useful in proportion as it tends to give people right ideas, which are essential to the foundation of right practice, and to remove wrong ideas, which are the no less essential foundations and fertile mothers of every description of error in practice. And inasmuch as, whatever practical people may say, this world is, after all, absolutely governed by ideas, and very often by the wildest and most hypothetical ideas, it is a matter of the very greatest importance that our theories of things, and even of things that seem a long way apart from our daily lives, should be as far as possible true, and as far as possible removed from error. It is not only in the coarser practical sense of the word "utility," but in this higher and broader sense that I measure the value of the study of biology by its utility, and I shall try to point out to you that you will feel the need of some knowledge of biology at a great many turns of this present nineteenth century life of ours. For example, most of us lay great and very just stress, upon the conception which is entertained of the position of man in this universe and his relation to the rest of nature. We have almost all of us been told, and most of us hold by the tradition, that man occupies an isolated and peculiar position in nature; that though he is in the world he is not of the world; that his relations to things

[1] See the distinction between the "sciences physiques" and the "sciences physiologiques" in the "Anatomie Générale," 1801.

[2] "Hydrogeologie," an. x. (1801).

[3] "The term *Biology*, which means exactly what we wish to express, *the Science of Life*, has often been used and has of late become not uncommon among good writers."—Whewell, "Philosophy of the Inductive Sciences," vol. i. p. 544 (edition of 1847).

about him are of a remote character, that his origin is recent, his duration likely to be short, and that he is the great central figure round which other things in this world revolve. But this is not what the biologist tells us. At the present moment you will be kind enough to separate me from them, because it is in no way essential to my argument just now that I should advocate their views. Don't suppose that I am saying this for the purpose of escaping the responsibility of their beliefs, because at other times and in other places I do not think that point has been left doubtful; but I want clearly to point out to you that for my present argument they may all be wrong; nevertheless, my argument will hold good. The biologists tells us that all this is an entire mistake. They turn to the physical organisation of man. They examine his whole structure, his bony frame, and all that clothes it. They resolve him into the finest particles into which the microscope will enable them to break him up. They consider the performance of his various functions and activities, and they look at the manner in which he occurs on the surface of the world. Then they turn to other animals and taking the first handy domestic animal—say a dog—they profess to be able to demonstrate that the analysis of the dog leads them, in gross, to precisely the same results as the analysis of the man; that they find almost identically the same bones, having the same relations; that they can name the muscles of the dog by the names of the muscles of the man, and the nerves of the dog by those of the nerves of the man, and that such structures and organs of sense as we find in the man such also we find in the dog; they analyse the brain and spinal cord, and they find that the nomenclature which fits the one answers for the other. They carry their microscopic inquiries in the case of the dog as far as they can, and they find that his body is resolvable into the same elements as those of the man. Moreover, they trace back the dog's and the man's development, and they find that, at a certain stage of their existence, the two creatures are not distinguishable the one from the other; they find that the dog and his kind have a certain distribution over the surface of the world comparable in its way to the distribution of the human species. What is true of the dog they tell us is true of all the higher animals; and they assert that for the whole of these creatures they can lay down a common plan, and regard the man and the dog, and the horse and the ox as minor modifications of one great fundamental unity. Moreover, the investigations of the last three-quarters of a century have proved, they tell us, that similar inquiries carried out through all the different kinds of animals which are met with in nature will lead us, not in one straight series, but by many roads, step by step, gradation by gradation, from man, at the summit, to specks of animated jelly at the bottom of the series; so that the idea of Leibnitz and of Bonnet, that animals form a great scale of being, in which there are a series of gradations from the most complicated form to the lowest and simplest; that idea, though not exactly in the form in which it was propounded by those philosophers, turns out to be substantially correct. More than this, when biologists pursue their investigations into the vegetable world, they find that they can, in the same way, follow out the structure of the plant from the most gigantic and complicated trees down, through a similar series of gradations, until they arrive at specks of animated jelly, which they are puzzled to distinguish from those specks which they reached by the animal road.

Thus, biologists have arrived at the conclusion that a fundamental uniformity of structure pervades the animal and vegetable worlds, and that plants and animals differ from one another simply as modifications of the same great general plan.

Again, they tell us the same story in regard to the study of function. They admit the large and important interval which, at the present time, separates the manifestations of the mental faculties observable in the higher forms of mankind, and even in the lower forms, such as we know them, mentally from those exhibited by other animals; but, at the same time, they tell us that the foundations or rudiments of almost all the faculties of man are to be met with in the lower animals; that there is a unity of mental faculty as well as of bodily structure, and that, here also, the difference is a difference of degree and not of kind. I said "almost all," for a reason. Among the many distinctions which have been drawn between the lower creatures and ourselves, there is one which is hardly ever insisted on,[1] but which may be very fitly spoken of in a place so largely devoted to art as that in which we are assembled. It is this, that while among various kinds of animals it is possible to discover traces of all the other faculties of man, especially the faculty of mimicry, yet that particular form of mimicry which shows itself in the imitation of form either by modelling or by drawing is not to be met with. As far as I know, there is no sculpture or modelling, and decidedly no painting or drawing, of animal origin. I mention the fact, in order that such comfort may be derived therefrom as artists may feel inclined to take.

If what the biologists tell us is true, it will be needful for us to get rid of our erroneous conceptions of man and of his place in nature, and substitute right ones for them. But it is impossible to form any judgment as to whether the biologists are right or wrong unless we are able to appreciate the nature of the arguments which they have to offer.

One would almost think that this was a self-evident proposition. I wonder what a scholar would say to the man who should undertake to criticise a difficult passage in a Greek play but who obviously had not acquainted himself with the rudiments of Greek grammar. And yet before giving positive opinions about these high questions of Biology people not only don't seem to think it necessary to be acquainted with the grammar of the subject, but they have not even mastered the alphabet. You find criticism and denunciation showered about by persons who not only have not attempted to go through the discipline necessary to enable them to be judges, but have not even reached that stage of emergence from ignorance in which the knowledge that such a discipline is necessary dawns upon the mind. I have had to watch with some attention—in fact I have been favoured with a good deal of it myself—the sort of criticism with which biologists and biological teachings are visited. I am told every now and then that there is a "brilliant article"[2] in so-and-so, in which we are all demolished. I used to read these things once, but I am getting old now, and I have ceased to attend very much to this cry of "wolf." When one does read any of these productions, what one finds generally, on the face of it, is that the brilliant critic is devoid of even the elements of biological knowledge, and that his brilliancy is like the light given out by the crackling of thorns under a pot of which Solomon speaks. So far as I recollect Solomon makes use of that image for purposes of comparison; but I won't proceed further into that matter.

Two things must be obvious: in the first place, that every man who has the interests of truth at heart must earnestly desire that every well-founded and just criticism that can be made should be made; but that, in the second place, it is essential to anybody's being able to benefit by criticism that the critic should know what he is talking about and be in a position to form a mental image of the facts symbolised by the words he uses. If not, it is as obvious in the case of a biological argument as it is in that

[1] I think that Prof. Allman was the first to draw attention to it.

[2] Galileo was troubled by a sort of people whom he called "paper philosophers," because they fancied that the true reading of nature was to be detected by the collation of texts. The race is not extinct, but, as of old, brings forth its "winds of doctrine" by which the weathercock heads among us are much exercised.

of a historical or philological discussion, that such criticism is a mere waste of time on the part of its author, and wholly undeserving of attention on the part of those who are criticised. Take it then as an illustration of the importance of biological study, that thereby alone are men able to form something like a rational conception of what constitutes valuable criticism of the teachings of biologists.[1]

Next, I may mention another bearing of biological knowledge—a more practical one in the ordinary sense of the word. Consider the theory of infectious disease. Surely that is of interest to all of us. Now the theory of infectious disease is rapidly being elucidated by biological study. It is possible to produce from among the lower animals cases of devastating diseases which have all the appearance of our infectious diseases, and which are certainly and unmistakably caused by living organisms. This fact renders it possible, at any rate, that that doctrine of the causation of infectious disease which is known under the name of "the germ theory" may be well-founded; and if so it must needs lead to the most important practical measures in dealing with those most terrible visitations. It may be well that the general as well as the professional public should have a sufficient knowledge of biological truths to be able to take a rational interest in the discussion of such problems, and to see, what I think they may hope to see, that, to those who possess a sufficient elementary knowledge of Biology, they are not all quite open questions.

Let me mention another important practical illustration of the value of biological study. Within the last forty years the theory of agriculture has been revolutionised. The researches of Liebig, and those of our own Lawes and Gilbert, have had a bearing upon that branch of industry the importance of which cannot be over-estimated; but the whole of these new views have grown out of the better explanation of certain processes which go on in plants, and which of course form a part of the subject-matter of Biology.

I might go on multiplying these examples, but I see that the clock won't wait for me, and I must therefore pass to the third question to which I referred:—Granted that Biology is something worth studying, what is the best way of studying it? Here I must point out that, since Biology is a physical science, the method of studying it must needs be analogous to that which is followed in the other physical sciences. It has now long been recognised that if a man wishes to be a chemist it is not only necessary that he should read chemical books and attend chemical lectures, but that he should actually for himself perform the fundamental experiments in the laboratory, and know exactly what the words which he finds in his books and hears from his teachers, mean. If he does not do that he may read till the crack of doom, but he will never know much about chemistry. That is what every chemist will tell you, and the physicist will do the same for his branch of science. The great changes and improvements in physical and chemical scientific education which have taken place of late have all resulted from the combination of practical teaching with the reading of books and with the hearing of lectures. The same thing is true in Biology. Nobody will ever know anything about Biology except in a dilettante "paper-philosopher" way, who contents himself with reading books on botany, zoology, and the like; and the reason of this is simple and easy to understand. It is that all language is merely symbolical of the things of which it treats; the more complicated the things, the more bare is the symbol, and the more its verbal definition requires to be supplemented by the information derived directly from the handling, and the seeing, and the touching of the thing symbolised:—that is really what is at the bottom of the whole matter. It is plain common sense, as all truth, in the long run is only common sense clarified. If you want a man to be a tea merchant, you don't tell him to read books about China or about tea, but you put him into a tea-merchant's office where he has the handling, the smelling, and the tasting of tea. Without the sort of knowledge which can be gained only in this practical way his exploits as a tea merchant will soon come to a bankrupt termination. The "paper-philosophers" are under the delusion that physical science can be mastered as literary accomplishments are acquired, but unfortunately it is not so. You may read any quantity of books, and you may be almost as ignorant as you were at starting, if you don't have, at the back of your minds, the change for words in definite images which can only be acquired through the operation of your observing faculties on the phenomena of nature.

It may be said:—"That is all very well, but you told us just now that there are probably something like a quarter of a million different kinds of living and extinct animals and plants, and a human life could not suffice for the examination of one-fiftieth part of all these." That is true, but then comes the great convenience of the way things are arranged; which is, that although there are these immense numbers of different kinds of living things in existence, yet they are built up, after all, upon marvellously few plans.

There are, I suppose, about 100,000 species of insects, if not more, and yet anybody who knows one insect—if a properly chosen one—will be able to have a very fair conception of the structure of the whole. I do not mean to say he will know that structure thoroughly or as well as it is desirable he should know it, but he will have enough real knowledge to enable him to understand what he reads, to have genuine images in his mind of those structures which become so variously modified in all the forms of insects he has not seen. In fact, there are such things as types of form among animals and vegetables, and for the purpose of getting a definite knowledge of what constitutes the leading modifications of animal and plant life it is not needful to examine more than a comparatively small number of animals and plants.

Let me tell you what we do in the biological laboratory in the building adjacent to this. There I lecture to a class of students daily for about four-and-a-half months, and my class have, of course, their text-books; but the essential part of the whole teaching, and that which I regard as really the most important part of it, is a laboratory for practical work, which is simply a room with all the materials arranged for ordinary dissection. We have tables properly arranged in regard to light, microscopes, and dissecting instruments, and we work through the structure of a certain number of animals and plants. As, for example, among the plants, we take a yeast plant, a *Protococcus*, a common mould, a *Chara*, a fern, and some flowering plant; among animals we examine such things as an amœba, a *vorticella*, and a fresh-water polype. We dissect a star-fish, an earthworm, a snail, a squid and a fresh-water mussel. We examine a lobster and a cray-fish, and a black beetle. We go on to a common skate, a cod-fish, a frog, a tortoise, a pigeon, and a rabbit, and that takes us about all the time we have to give. The purpose of this course is not to make skilled dissectors, but to give every student a clear

[1] Some critics do not even take the trouble to read. I have recently been adjured with much solemnity, to state publicly why I have "changed" my opinion as to the value of the palæontological evidence of the occurrence of evolution.

To this my reply is, Why should I when that statement was made seven years ago? An address delivered from the Presidential Chair of the Geological Society in 1870 may be said to be a public document, inasmuch as it not only appeared in the *Journal* of that learned body, but was re-published in 1873 in a volume of "Critiques and Addresses," to which my name is attached. Therein will be found a pretty full statement of my reasons for enunciating two propositions: (1) that "when we turn to the higher *Vertebrata*, the results of recent investigations, however we may sift and criticise them, seem to me to leave a clear balance in favour of the evolution of living forms one from another;" and (2) that the case of the horse is one which "will stand rigorous criticism."

Thus I do not see clearly in what way I can be said to have changed my opinion, except in the way of intensifying it, when in consequence of the accumulation of similar evidence since 1870, I recently spoke of the denial of evolution as not worth serious consideration.

and definite conception, by means of sense-images, of the characteristic structure of each of the leading modifications of the animal kingdom; and that is perfectly possible, by going no further than the length of that list of forms which I have enumerated. If a man knows the structure of the animals I have mentioned, he has a clear and exact, however limited, apprehension of the essential features of the organisation of all those great divisions of the animal and vegetable kingdoms to which the forms I have mentioned severally belong. And it then becomes possible for him to read with profit, because every time he meets with the name of a structure, he has a definite image in his mind of what the name means in the particular creature he is reading about, and therefore the reading is not mere reading. It is not mere repetition of words; but every term employed in the description, we will say, of a horse or of an elephant, will call up the image of the things he had seen in the rabbit, and he is able to form a distinct conception of that which he has not seen as a modification of that which he has seen.

I find this system to yield excellent results; and I have no hesitation whatever in saying, that any one who has gone through such a course, attentively, is in a better position to form a conception of the great truths of Biology, especially of morphology (which is what we chiefly deal with), than if he had merely read all the books on that topic put together.

The connection of this discourse with the Loan Collection of Scientific Apparatus arises out of the exhibition in that collection of certain aids to our laboratory work. Such of you as have visited that very interesting collection may have noticed a series of diagrams and of preparations illustrating the structure of a frog. Those diagrams and preparations have been made for the use of the students in the biological laboratory. Similar diagrams and preparations illustrating the structure of all the other forms of life we examine, are either made or in course of preparation. Thus the student has before him, first, a picture of the structure he ought to see, secondly, the structure itself worked out; and if with these aids, and such needful explanations and practical hints as a demonstrator can supply, he cannot make out the facts for himself in the materials supplied to him, he had better take to some other pursuit than that of biological science.

I should have been glad to have said a few words about the use of museums in the study of Biology, but I see that my time is becoming short, and I have yet another question to answer. Nevertheless I must, at the risk of wearying you, say a word or two upon the important subject of museums. Without doubt there are no helps to the study of Biology, or rather to some branches of it, which are, or may be, more important than natural history museums; but, in order to take this place in regard to Biology, they must be museums of the future. The museums of the present do not do by any means so much for us as they might do. I do not wish to particularise, but I dare say many of you seeking knowledge, or in the laudable desire to employ a holiday usefully, have visited some great natural history museum. You have walked through a quarter of a mile of animals more or less well stuffed, with their long names written out underneath them, and, unless your experience is very different from that of most people, the upshot of it all is that you leave that splendid pile with sore feet, a bad headache, and a general idea that the animal kingdom is a "mighty maze without a plan." I do not think that a museum which brings about this result does all that may be reasonably expected of such an institution. What is needed in a collection of natural history is that it should be made as accessible and as useful as possible, on the one hand to the general public, and on the other to scientific workers. That need is not met by constructing a sort of happy hunting-ground of miles of glass cases, and, under the pretence of exhibiting everything, putting the maximum amount of obstacle in the way of those who wish properly to see anything.

What the public want is easy and unhindered access to such a collection as they can understand and appreciate; and what the men of science want is similar access to the materials of science. To this end the vast mass of objects of natural history should be divided into two parts—one open to the public, the other to men of science, every day. The former division should exemplify all the more important and interesting forms of life. Explanatory tablets should be attached to them, and catalogues containing clearly-written popular expositions of the general significance of the objects exhibited should be provided. The latter should contain, packed into a comparatively small space, in rooms adapted for working purposes, the objects of purely scientific interest. For example, we will say I am an ornithologist. I go to examine a collection of birds. It is a positive nuisance to have them stuffed. It is not only sheer waste, but I have to reckon with the ideas of the bird-stuffer, while, if I have the skin and nobody has interfered with it I can form my own judgment as to what the bird was like. For ornithological purposes what is needed is not glass cases full of stuffed birds on perches, but convenient drawers into each of which a great quantity of skins will go. They occupy no great space and do not require any expenditure beyond their original cost. But for the purpose of the public, who want to learn indeed, but do not seek for minute and technical knowledge, the case is different. What one of the general public walking into a collection of birds desires to see is not all the birds that can be got together. He does not want to compare a hundred species of the sparrow tribe side by side; but he wishes to know what a bird is, and what are the great modifications of bird structure, and to be able to get at that knowledge easily. What will best serve his purpose is a comparatively small number of birds carefully selected, and artistically, as well as accurately, set up; with their different ages, their nests, their young, their eggs, and their skeletons side by side; and in accordance with the admirable plan which is pursued in this museum, a tablet, telling the spectator in legible characters what they are and what they mean. For the instruction and recreation of the public such a typical collection would be of far greater value than any many-acred imitation of Noah's ark.

Lastly comes the question as to when biological study may best be pursued. I do not see any valid reason why it should not be made, to a certain extent, a part of ordinary school training. I have long advocated this view, and I am perfectly certain that it can be carried out with ease, and not only with ease, but with very considerable profit to those who are taught; but then such instruction must be adapted to the minds and needs of the scholars. They used to have a very odd way of teaching the classical languages when I was a boy. The first task set you was to learn the rules of the Latin grammar in the Latin language—that being the language you were going to learn! I thought then that this was an odd way of learning a language, but did not venture to rebel against the judgment of my superiors. Now, perhaps, I am not so modest as I was then, and I allow myself to think that it was a very absurd fashion. But it would be no less absurd if we were to set about teaching Biology by putting into the hands of boys a series of definitions of the classes and orders of the animal kingdom, and making them repeat them by heart. That is a very favourite method of teaching, so that I sometimes fancy the spirit of the old classical system has entered into the new scientific system, in which case I would much rather that any pretence at scientific teaching were abolished altogether. What really has to

be done is to get into the young mind some notion of what animal and vegetable life is. You have to consider in this matter practical convenience as well as other things. There are difficulties in the way of a lot of boys making messes with slugs and snails; it might not work in practice. But there is a very convenient and handy animal which everybody has at hand, and that is himself; and it is a very easy and simple matter to obtain common plants. Hence the broader facts of anatomy and physiology can be taught to young people in a very real fashion by dealing with the broad facts of human structure. Such viscera as they cannot very well examine in themselves, such as hearts, lungs, and livers, may be obtained from the nearest butcher's shop. In respect to teaching something about the biology of plants, there is no practical difficulty, because almost any of the common plants will do, and plants do not make a mess—at least they do not make an unpleasant mess; so that, in my judgment, the best form of Biology for teaching to very young people is elementary human physiology on the one hand, and the elements of botany on the other; beyond that I do not think it will be feasible to advance for some time to come. But then I see no reason why in secondary schools, and in the Science Classes which are under the control of the Science and Art Department—and which I may say, in passing, have, in my judgment, done so very much for the diffusion of a knowledge over the country—I think that in those cases we may go further, and we may hope to see instruction in the elements of Biology carried out, not perhaps to the same extent, but still upon somewhat the same principle as we do here. There is no difficulty, when you have to deal with students of the ages of 15 or 16, in practising a little dissection and getting a notion, at any rate, of the four or five great modifications of the animal form, and the like is true in regard to plants.

While, lastly, to all those who are stud ıg biological science with a view to their own edification merely, or with the intention of becoming zoologists or botanists; to all those who intend to pursue physiology—and especially to those who propose to employ the working years of their lives in the practice of medicine—I say that there is no training so fitted, or which may be of such important service to them, as the thorough discipline in practical biological work which I have sketched out as being pursued in the laboratory hard by.

I may add that, beyond all these different classes of persons who may profit by the study of Biology, there is yet one other. I remember, a number of years ago, that a gentleman who was a vehement opponent of Mr. Darwin's views and had written some terrible articles against them, applied to me to know what was the best way in which he could acquaint himself with the strongest arguments in favour of evolution. I wrote back, in all good faith and simplicity, recommending him to go through a course of comparative anatomy and physiology, and then to study development. I am sorry to say he was very much displeased, as people often are with good advice. Notwithstanding this discouraging result, I venture, as a parting word, to repeat the suggestion, and to say to all the more or less acute lay and clerical "paper-philosophers"[1] who venture into the regions of biological controversy—Get a little sound, thorough, practical, elementary instruction in biology.

T. H. HUXLEY

[1] Writers of this stamp are fond of talking about the Baconian method. I beg them therefore to lay to heart these two weighty sayings of the herald of Modern Science:—

"Syllogismus ex propositionibus constat, propositiones ex verbis, verba notionum tesseræ sunt. Itaque si notiones ipsæ (*id quod basis rei est*) confusæ sint et temere a rebus abstractæ, nihil in iis quæ superstruuntur est firmitudinis."—"Novum Organon," ii. 14.

"Huic autem vanitati nonnulli ex modernis summa levitate ita indulserunt, ut in primo capitulo Geneseos et in libro Job et aliis scripturis sacris, philosophiam naturalem fundare conati sint; *inter vivos quærentes mortua.*"—*Ibid.*, 65.

EXPERIMENTS WITH THE RADIOMETER

I.

ABSTRACTS of my earlier papers on "Repulsion Resulting from Radiation" having appeared in NATURE, it has been suggested that an account of my later researches, which place the subject in such a different light, may also prove of interest.

It has already been shown that if the air is expelled from a large bulb containing a suspended bar of pith, and a lighted candle is placed about 2 inches from the globe, the pith bar commences to oscillate to and fro, the swing gradually increasing in amplitude until the dead centre is passed over, when several complete revolutions are made. The torsion of the suspended fibre now offers resistance to the revolutions, and the bar commences to turn in the opposite direction. It has been found, however, that very little movement takes place until the vacuum is so good as to be almost beyond the powers of an ordinary air-pump to produce, and that, as the vacuum gets more nearly absolute, so the force increases in power. The most obvious explanation therefore is, that the repulsive action is due to radiation; but at a very early stage of my investigation I found that the best vacuum I had succeeded in producing might contain enough matter to offer resistance to motion, and in describing an experiment in a paper sent to the Royal Society on February 5, 1876, I said that the impression conveyed to my mind was that the torsion beam was swinging in a viscous fluid, and the repulsion caused by radiation was indirectly due to a difference of thermometric heat between the black and white surfaces of the moving body, and that it might be due to a secondary action on the residual gas.

I have recently succeeded in producing such a complete exhaustion that I have not only reached the point of maximum effect, but gone so far beyond it that repulsion nearly ceases, and the results I have thus obtained seem to show conclusively that the true explanation of the action of the radiometer is that given by Mr. Johnstone Stoney, according to which the repulsion is due to the internal movements of the molecules of the residual gas. When the mean length of path between successive collisions of the molecules is small compared with the dimensions of the vessel, the molecules, rebounding from the heated surface, and therefore moving with an extra velocity, help to keep back the more slowly moving molecules which are advancing towards the heated surface; it thus happens that though the individual kicks against the heated surface are increased in strength in consequence of the heating, yet the number of molecules struck is diminished in the same proportion, so that there is equilibrium on the two sides of the discs, even though the temperature of the faces are unequal. But when the exhaustion is carried to so high a point that the molecules are sufficiently few, and the mean length of path between their successive collisions is comparable with the dimensions of the vessel, the swiftly-moving, rebounding molecules spend their forces in part or in whole on the sides of the vessel; and the onward crowding, more slowly-moving molecules are not kept back as before, so that the number which strike the warmer face approaches to, and in the limit equals, the number which strike the back cooler face; and as the individual impacts are stronger on the warmer than on the cooler face, pressure is produced, causing the warmer face to retreat.

Before referring at length to the experiments which led to my adopting the above theory, I will describe some effects of dark heat, &c., on the radiometer. In a paper I sent to the Royal Society on January 5, 1876, and which is now being published in the *Philosophical Transactions* of the Royal Society, about seventeen pages are occupied with the description of my experiments with various forms of this instrument. In the present paper I propose only to refer to a few typical experiments made during the year 1875.

To show the action of dark heat on the radiometer, a candle was placed at such a distance from the instrument that the arms would make one revolution a minute. A small glass flask of boiling water was then placed half-an-inch from the bulb. The revolutions instantly stopped, two of the arms setting equidistant from the hot-water flask. The flask of water was removed. As the portion of the bulb which had been heated by the hot water cooled, the white surface gradually crept nearer and nearer to it, the superior repulsion of the candle on the black discs urging the arms round, and acting in opposition to the repulsion of the hot glass to the white disc. At last the force of the light drove the white disc with difficulty past the hot spot of glass. Rotation then commenced, but for some revolutions there appeared to be a difficulty in the white discs passing the spot of glass which had been warmed by the hot water; and the flask of boiling water being replaced in its position half-an-inch from the bulb of the radiometer, the rotation immediately stopped.

The instrument having become cool, the candle was again placed in position, so that it produced one revolution in a minute. The finger was then pressed against the side of the bulb, and as the spot of glass got warm, the white surface experienced more and more difficulty in getting past it, until at last one refused to pass, and the arms came to rest.

The instrument was again allowed to cool, and the revolutions recommenced at the usual speed (the laboratory in which this was tried was somewhat cold). I then came from a warm room, and stood a foot from the radiometer, watching it. In about a minute the radiant heat from my body had warmed the side of the bulb nearest to me sufficiently to cause an appreciable difficulty in the movement, and soon the revolutions stopped. The same effect has been observed if the radiometer is brought into a very warm room and placed near a cold window. If the daylight is feeble, the instrument not very sensitive, or an observer stands near the instrument, an appreciable sticking is observed as the white discs come near that part of the bulb which is the warmest.

These experiments show that many precautions are necessary to guard against the interfering action of unequal heating of the radiometer when it is being used for accurate measurements.

Having found such an antagonistic action of dark heat, I tried the action of ice. This is equivalent to warming the opposite side of the instrument. A lump of ice was brought within half an inch of the bulb on the opposite side to the candle. The revolutions got slower, until at last the movement stopped altogether, one arm pointing direct to the ice, and being apparently held there by a powerful attractive force. Bringing the candle nearer caused the arms to oscillate a little, and when it was almost close to the bulb the force of the light overcame the action of the ice, and the arms revolved again, but irregularly, and with jerks, the discs moving quickly to the ice and leaving it with difficulty. In this action of ice no preference was noticed for either the black or white surface.

A very delicate radiometer, in 2-inch bulb, was placed in a sufficient light to allow it to be seen distinctly, but not enough to cause it to move. I then came out of a warm room and stood near it. In a few seconds it began to move slowly round in the negative direction, *i.e.* the black discs advanced instead of retreated. On moving away from the instrument the rotation gradually stopped. I again approached it, and held one hand an inch from the bulb. Rotation soon commenced, but still in the reverse way. These experiments were frequently repeated and always with the same results.

When the instrument was at rest I came quickly to it, and gently breathed on the bulb. There was a slight movement in the normal direction, but this stopped directly, and the arms then revolved the reverse way for more than a minute, performing three or four complete revolutions.

A glass shade four inches in diameter was held over a gas-flame till the air inside was warm, and the inner surface dim with steam. It was then inverted over the radiometer. Rotation commenced the reverse way, and kept up for several minutes. The glass shade was then dried inside, and heated uniformly before a fire until it had a temperature of about 50° C. It was then inverted over the radiometer. Reverse rotation instantly commenced, and kept up with some vigour for more than five minutes, diminishing in speed until the shade had cooled down to the temperature of the surrounding air.

The same experiment was repeated, and whilst the arms were in full negative rotation, a lighted candle was slowly brought near it. When three feet off the negative rotation slackened. When the candle was about two feet off the arms became still, and when nearer than two feet the instrument rotated normally, the antagonism between the action of the hot shade and the lighted candle was perfect; by moving the candle to and fro it was easy to cause the radiometer to move in one direction or the other, or to become still.

I now tried the action of a radiometer the moving parts of which were made of a good conductor of heat, such as a metal, instead of pith, which is a bad conductor of heat. I selected thin rolled brass as the material wherewith to make the arms and discs of a radiometer. The parts were all fastened together with hard solder, and no cement or organic matter was used, so that if necessary the instrument could be submitted to a high temperature without injury. The moving portion weighed 13·1 grains. One side of the discs was silvered and polished, the other side being coated with lampblack. The apparatus was exhausted with a charcoal reservoir attached. A candle $1\frac{1}{2}$ inch from the bulb caused it to revolve about once a second, the black surface being repelled in the normal manner.

Standing in a rather dark cold room, it was covered with a warm glass shade, and it immediately began to revolve the negative way, but very slowly. A few drops of ether poured on the bulb caused the arms to move rather rapidly the normal way. A hot shade put over whilst it was thus moving caused it to stop, and then begin moving the reverse way. A small non-luminous gas flame was held vertically beneath the apparatus, so that hot air should ascend and wrap round the bulb on all sides. The arms now revolved the reverse way.

The brass radiometer being somewhat heavy, one was made of aluminium, the moving parts being hard soldered as before. A siphon-gauge was attached, and the apparatus connected direct on to the pump by a spiral, no charcoal tube being used. One side of the wings was bright aluminium, and the other was lampblacked. When exhausted the arms revolved very quickly to a candle a few inches off, the black being repelled. On removing the candle the arms stopped and immediately commenced revolving the reverse way, keeping up rotation for more than ten minutes, and being little inferior in speed to what it was when the candle shone on it. The whole of the bulb was heated with a Bunsen burner; whilst it was getting hot the aluminium arms revolved rapidly in the normal direction, but as soon as the source of heat was removed and cooling commenced, rotation set up in the reverse way, and continued with great energy till the whole thing was cold. It appeared as if the reverse movement during the cooling was equal in energy to the normal movement as it was being heated.

A little ether was poured on the bulb of a very sensitive pith radiometer as it was standing still in a faint light. The evaporation of the ether caused a chilling of the instrument and a rapid abstraction of heat from the arms. They commenced to move in the normal direction and

increased quickly in speed until they revolved at a rate of one in four seconds. This movement kept up for several minutes, and as it slackened it could at any time be revived by a few drops of ether on the bulb. When in rapid movement a hot glass shade was placed over the radiometer the movement slackened, the arms quickly came to rest and ,immediately revolved in the reverse direction, acquiring a speed of about two revolutions a minute, and keeping up this reverse movement for more than ten minutes.

I again set the instrument in rapid rotation by dropping ether on the top of the bulb and applied the tip of one finger to the side of the bulb for ten seconds. The rotation stopped, and I could not start it again for some minutes, although I dropped ether on the bulb, several times in the interval. When the radiometer had once more acquired the temperature of the air I dropped ether on the bulb, not in the centre, but so that the ether wetted only half of the bulb. The arm which was nearest to the part most chilled by the ether rushed towards that part and remained, as it were, fixed opposite to it, refusing to move away, although I tried to equalise the temperature by dropping ether on the other parts of the bulb, and to drive it round by bringing a candle near. Not until the candle came within six inches of the bulb did the arms begin to rotate, which they then did with a rush, as if suddenly relieved from a state of tension.

I have referred to a sufficient number of experiments to show that a metal radiometer rotates in a negative direction on being exposed to the action of dark heat, the black advancing and continuing to do so until the temperature has become uniform throughout. On removing the source of heat, the fly commences to revolve with rapidity the positive way, the black this time retreating as it would if light shone on it.

To determine whether at temperatures between 250° and 100° the repellant action of radiant heat was about equal on black and on white surfaces, I used a radiometer having pith discs blackened on one side. A tube was sealed into one side of the bulb, and having two stout platinum wires passing along it, sealed their whole length in glass to prevent leakage of air into the interior of the apparatus. At the ends of the wires a spiral of fine platinum wire was fastened, and the other ends terminated in loops outside. The bulb was perfectly exhausted, and the following experiments were tried :—

A resistance-coil was so adjusted that a battery would keep the platinum spiral at a bright red heat. The arms of the radiometer, which were before quite still, moved rapidly until two of the discs were one on each side of the hot spiral, the black disc being further off than the white disc. The resistance was then gradually increased, and as the temperature of the spiral diminished, the black disc gradually approached the spiral, until, when the temperature was just at the point of visible redness in a dark room, the black and white discs were practically equidistant from the spiral. On diminishing the resistance, the same phenomena took place in inverse order.

The resistance was again adjusted to give a bright red spiral, and the contact key kept pressed down. A lighted match was momentarily brought near the bulb, so as to start a movement. Rotation of the arms commenced, and kept up, with some energy, at the rate of about one revolution in five seconds, equal to that given by a candle eight inches off. There was some little hesitation, as the white side came up to the spiral, but this was scarcely noticed when the speed had become steady. The resistance was now slightly increased. The speed became slower as the temperature of the spiral diminished, and the hesitation, as the white approached the spiral, became more apparent. The resistance was further increased, with the effect of making rotation still slower, bringing the temperature of the spiral down to just visible redness in the dark. The speed of rotation again slackened ; at each approach of the white surface to the spiral it appeared to stop, hesitate, and then get past with a rush. Thus it went on for a few revolutions, until one white disc, a little nearer, perhaps, than the others, was not able to pass, and the arms, after a few oscillations, came to rest, the black and the white surfaces being, as near as I could judge, equidistant from the hot spiral.

I now tried to ascertain whether, at temperatures lower than 100° C., the white would be repelled most.

The resistance of the coil was increased again, and the position of the arms in respect to the spiral noticed. When so much resistance was offered to the passage of the current that the spiral would only be just warm, I fancied the white was further from it than the black, but the observation was not satisfactory at higher temperatures ; up to visible redness the repulsion was equal for each. Breathing on the bulb sent the arms rapidly round the reverse way.

The battery was disconnected from the instrument, and one end of a wire was attached to one of the platinum loops, the other end of the wire being connected to the prime conductor of a frictional electrical machine. A few turns of the handle sent the arms flying about wildly, first in the positive and then in the negative direction, till finally one pointed steadily to the platinum spiral, and refused to move. When the candle was quite close it overcame the interference, and the discs revolved in an irregular jerky manner. In three or four days the electrical disturbance was sufficiently diminished to enable me to proceed with my experiments, but I could detect the influence for weeks after.

One pole of a small induction-coil capable of giving half-inch sparks in air, was fastened to the platinum loops, the other pole being held by an insulating handle. The loose pole was then brought near the bulb. The nearest disc rushed round to it and followed it a little, then it stuck as if the glass were electrified. By gently moving the loose pole round I could get the arms to rotate in either direction, and they would keep on for five minutes or more when once started. These movements appear all to be explained by the known laws of static electricity, the rotations being of the "electrical fly" kind.

I obtained rotation in a radiometer without having the surfaces of the discs differently coloured. One having the pith discs lamp-blacked on both sides, and weighing 1·25 grain, was exhausted with a charcoal tube attached. On a candle being brought near it, the arms moved until two of the discs were equidistant from the flame, and no amount of initial impulse in either direction would set it in rotation. A piece of ice caused it to move until one disc pointed to the ice, when it also stopped, but by shading the candle with a screen, so that the light shone on only one half of the tube, rapid rotation commenced, which, by altering the position of the screen to the other side, was instantly stopped, and changed into as rapid rotation in the opposite direction.

To enable me to exhibit the movement of a radiometer to a large audience I have made an instrument, the discs of which are of thin glass, silvered and polished on one side, and coated with lampblack on the other. Owing to its great weight the movement is somewhat slow, but in the sun, or, with a strong light shining on the instrument, it is very striking, as it shows discs of light chasing each other round the room.

To communicate motion from the interior of the bulb to the outside, a radiometer was made which would carry round a magnet. Outside the bulb of this instrument I suspended, in a vertical position, a smaller magnet having the south pole at the top and the north pole at the bottom ; this oscillates to and fro with every revolution of the radiometer, and making contact at the bottom, carries an electric current from a battery to a Morse instrument through which a ribbon of paper is drawn by clockwork

so that at each revolution of the radiometer a record is printed on the strip of paper by dots; close together if the radiometer revolves quickly, farther apart if it goes slower.

The power of the earth on the magnet is too great to allow the radiometer to start without some initial impetus; there should therefore be an astatic combination inside the bulb, but for a single experiment it may be set going by placing a few coils of insulated copper wire outside the bulb and depressing the battery key for an instant. An electric current is thus passed through the coils of wire, and the interior magnet is immediately deflected from its north-south position; the impetus thus gained enables the light to keep up the rotation.

For the purpose of measuring the amount of force exerted by radiation I constructed a torsion balance capable of indicating the millionth of a grain. A light beam having two square inches of pith at one end is balanced on a fine fibre of glass[1] stretched horizontally in a tube, one end of the fibre being connected with a torsion handle passing through the tube, and indicating angular movements on a graduated circle. The beam is cemented to the torsion fibre, and the whole is enclosed in glass and connected with the mercury pump by a spiral tube and exhausted as perfectly as possible. A flat oblong piece of soft iron weighing accurately 0·01 grain is put into the cross tube under the pith surface. This weight can be picked up by a horse-shoe magnet outside the tube and dropped on any part of the pith. A mark is made at the exact centre of the pith surface, and by moving the magnet about it is easy to place the iron weight accurately on this mark. A ray of light from a lamp reflected from a mirror in the centre of the beam to a millimetre scale four feet off shows the slightest movement. When the reflected ray points to zero, a turn of the torsion handle in one direction or the other will raise or depress the pith end of the beam, and thus cause the index ray to travel along the scale to the right or to the left. If a small weight is placed on one end so as to depress it, and the torsion handle is then turned, the tendency of the glass fibre to untwist itself will ultimately balance the downward pressure of the weight, and will again bring the index ray to zero. It was found that when the weight of the 1-100th of a grain was placed on the pith surface the torsion handle had to be turned twenty-seven revolutions and 353°, or 10073° before the beam became horizontal. The downward pressure of the 1-100th of a grain was therefore equivalent to the force of torsion of the glass thread when twisted through 10073°.

I then found out the degree of delicacy of the balance. 1° of torsion gave a very decided movement of the index ray, a torsion of 10073° balancing the 1-100th of a grain, while 100074° overbalanced it. The balance will therefore turn to the 99-100,000,000th of a grain.

Weighed in this balance, the mechanical force of a candle 12 inches off was found to be 0·000444 grain; of a candle 6 inches off 0·001772 grain. At half the distance the weight of radiation should be four times, or 0·001776 grain; the difference between theory and experiment being only four millionths of a grain is a sufficient proof that the indications of this instrument follow rigidly the law of inverse squares. An examination of the differences between the separate observations and the mean shows that my estimate of the sensitiveness of this balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

I performed an experiment at the meeting of the Royal Society on March 30 last to demonstrate the movement of the glass case of the radiometer. I made use of a large radiometer in a 4-inch bulb with ten arms, eight of which were brass, and the other two a long watch-spring magnet. The discs were of pith blackened on one side.

The instrument was floated in a vessel of water, four candles being placed round it to set the arms in rotation. A mark was put on the glass envelope to enable a slight movement to be seen.

A powerful magnet was now brought near the moving arms, which immediately stopped, and at the same time the glass envelope commenced to revolve in the opposite direction to that in which the arms had been revolving. The movement kept up as long as the candles were burning, and the speed was one revolution in two minutes. On the magnet being removed the arms obeyed the force of radiation from the candles and revolved rapidly, whilst the glass envelope quickly came to rest. The candles were then blown out, and as soon as the whole instrument had come to rest, a bar-magnet was moved alternately from one side to the other of the radiometer, so as to cause the vanes to rotate as if they had been under the influence of a candle. The glass envelope moved about one revolution in three minutes in the same direction as the arms, and on reversing the direction of movement of arms, the glass envelope changed direction also. This I consider is proof that the internal friction, either of the steel point on the glass socket or the vanes against the residual air, or of both these causes combined, is considerable. Moving the vanes round by the exterior magnet carries the whole envelope round in opposition to the friction of the water against the glass.

In another communication I propose to give the results of my experiments on the influence of the residual gas on the movement of the radiometer, and also refer to other results which I have recently obtained.

WILLIAM CROOKES

[1] The torsion of fibre must be selected with great care. Ten threads were drawn out before the blowpipe and suspended from a horizontal beam. Weights were then gradually hung on to the lower ends. Only two were found strong enough The one selected stood 450 grains without breaking, its diameter being less than ·001 inch.

ON A NEW ASTRONOMICAL CLOCK[1]

THE object of this communication was to explain to members of the Association and give them an opportunity of seeing in my house in the University a clock which had been described in a communication to the Royal Society, in 1869, entitled "On a New Astronomical Clock and a Pendulum Governor for Uniform Motion." The following description is taken from the *Proceedings* of the Royal Society for 1869, except a few alterations and additions, and except the drawings, which have not been hitherto published:—

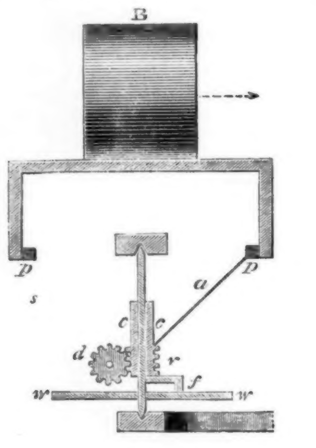
It seems strange that the dead-beat escapement should still hold its place in the astronomical clock, when its geometrical transformation, the cylinder escapement of the same inventor, Graham, only survives in Geneva watches of the cheaper class. For better portable timekeepers it has been altered, through the vicious rack-and-pinion movement, into the superlatively good detached lever. If it is possible to make astronomical clocks go better than at present by merely giving them a better escapement, it is quite certain that one on the same principle as the detached lever, or as Earnshaw's ship-chronometer escapement, would improve their timekeeping.

But the irregularities hitherto tolerated in astronomical clocks may be due more to the faultiness of the steel and mercury compensation pendulum, with its loosely attached glass jar, and of the mode in which it is hung, and to instability of the supporting clock-case or framework, than to imperfection of the escapement and the greatness of the arc of vibration which it requires; therefore it would be wrong to expect confidently much improvement in the time-keeping merely from improvement of the escapement. I have therefore endeavoured to improve both the compensation for change

[1] "On a New Form of Astronomical Clock with Free Pendulum and Independently Governed Uniform Motion for Escapement Wheel." By Prof. Sir William Thomson, F.R.S. (Communicated to Section A of the British Association, Thursday, September 7, 1876.)

of temperature in the pendulum, and the mode of its support, in a clock which I have recently made with an escapement on a new principle, in which the simplicity of the dead-beat escapement of Graham is retained, while its great defect, the stopping of the whole train of wheels by pressure of a tooth upon a surface moving with the pendulum, is remedied.

Imagine the escapement-wheel of a common dead-beat clock to be mounted on a collar fitting easily upon a shaft, instead of being rigidly attached to it. Let friction be properly applied between the shaft and the collar, so that the wheel shall be carried round by the shaft unless resisted by a force exceeding some small definite amount; and let a governor giving uniform motion be applied to the train of wheel-work connected with this shaft, and so adjusted that, when the escapement-wheel is unresisted, it will move faster by a small percentage than it must move to keep time properly. Now let the escapement wheel, thus mounted and carried round, act upon the escapement, just as it does in the ordinary clock. It will keep the pendulum vibrating, and will,



just as in the ordinary clock, be held back every time it touches the escapement during the interval required to set it right again from having gone too fast during the preceding interval of motion. But in the ordinary clock the interval of rest is considerable, generally greater than the interval of motion. In the new clock it is equal to a small fraction of the interval of motion: $\frac{1}{300}$ in the clock as now working, but to be reduced probably to something much smaller yet. The simplest appliance to count the turns of this escapement-wheel (a worm, for instance, working upon a wheel with thirty teeth, carrying a hand round, which will correspond to the seconds' hand of the clock) completes the instrument; for minute and hour-hands are a superfluity in an astronomical clock.

In various trials which I have made since the year 1865, when this plan of escapement first occurred to me, I have used several different forms, all answering to the preceding description, although differing widely in their geometrical and mechanical characters. In all of them the escapement-wheel is reduced to a single tooth or arm, to diminish as much as possible the moment of inertia of the mass stopped by the pendulum. This arm revolves in the period of the pendulum (two seconds for a one second's pendulum), or in some odd multiple of it. Thus the pendulum may execute one or more complete periods of vibration without being touched by the escapement. In all my trials the pallets have been attached to the bottom of the pendulum, projecting below it, in order that satisfactory action with a very small arc of vibration (not more on each side than $\frac{1}{100}$ of the radius, or 1 centimetre for the seconds' pendulum) may be secured.

In the clock in my house the seconds' pendulum of the fine movement, vibrates with great constancy through half a millimetre, that is to say, through an arc of $\frac{1}{200}$ of the radian, on each side of the vertical. This, I believe, is the smallest range that has hitherto been realised in any seconds' pendulum of an astronomical or other clock.

In the drawing *s* represents the vertical escapement shaft, round which is fitted loosely the collar *c*, carrying the worm *v*. The small wheel, *d*, is worked by *v*, and carries round the seconds' hand of the clock. *a* represents a piece of fine steel wire, being the single arm to which the teeth of the escapement-wheel are reduced in the clock described in this paper; *p p* the pallets attached to bars projecting downwards from the bob, B, of the pendulum; *f*, a foot bearing the weight of the collar-worm and escapement tooth. The bar connecting *f* with the collar is of such a length as to give a proper moment to the frictional force by which the collar is carried round. The shaft *s* carries a wheel, represented in section by *w w*, which is driven by a train of wheel-work (not shown in the drawing) from the governor. This wheel is made to go $\frac{1}{3}$ per cent. faster than once round in two seconds, while the pendulum prevents the collar from going round more than once in two seconds.

My trials were rendered practically abortive from 1865 until a few months ago by the difficulty of obtaining a satisfactory governor for the uniform motion of the escapement-shaft; this difficulty is quite overcome in the pendulum governor, which I now proceed to describe.

Imagine a pendulum with single-tooth escapement mounted on a collar loose on the escapement shaft just as described above—the shaft being vertical in this case also. A square-threaded screw is cut on the upper quarter of the length of the shaft, this being the part of it on which the escapement-collar works; and a pin fixed to the collar projects inwards to the furrow of the screw, so that, if the collar is turned relatively to the shaft, it will be carried along, as the nut of a screw, but with less friction than an ordinary nut. Below the screw and long nut-collar, three-quarters of the length of the escapement-shaft is surrounded by a tube which, by wheel-work, is carried round about 5 per cent. faster than the central shaft. This outer shaft, by means of friction produced by the pressure of proper springs, carries the nut collar round along with it, except when the escapement-tooth is stopped by either of the pallets attached to the pendulum. A stiff cross-piece (like the head of a T), projecting each way from the top of the tubular shaft, carries, hanging down from it, the governing masses of a centrifugal friction governor. These masses are drawn towards the axis by springs, the inner ends of which are acted on by the nut collar, so that the lower or the higher the latter is in its range, the springs pull the masses inwards with less or more force. A fixed metal ring coaxial with the main shaft holds the governing masses in when their centrifugal forces exceed the forces of the springs, and resists the motion by forces of friction increasing approximately in simple proportion to the excess of the speed above that which just balances the forces of the springs. As long as the escapement-tooth is unresisted, the nut collar is carried round with the quicker motion of the outer tubular shaft, and so it *screws upwards*, increasing the force of the springs. Once every semiperiod of the pendulum it is held back by either

pallet, and the nut-collar screws *down* as much as it rose during the preceding interval of freedom when the action is regular; and the central or main escapement-shaft turns in the same period as the tooth, being the period of the pendulum. If through increase or diminution of the driving-power, or diminution or increase of the coefficient of friction between the governing masses and the ring on which they press, the shaft tends to turn faster or slower, the nut collar works its way down or up the screw, until the governor is again regulated, and gives the same speed in the altered circumstances. It is easy to arrange that a large amount of regulating power shall be implied in a single turn of the nut collar relatively to the central shaft, and yet that the periodic application and removal of about $\frac{1}{30}$ of this amount in the half period of the pendulum shall cause but a *very small* periodic variation in the speed. The latter important condition is secured by the great moment of inertia of the governing masses themselves round the main shaft. My communication to the Royal Society ended as follows:—

"I hope after a few months' trial, to be able to present a satisfactory report of the performance of the clock now completed according to the principles explained above. As many of the details of execution may become modified after practical trial, it is unnecessary that I should describe them minutely at present. Its general appearance, and the arrangement of its characteristic parts, may be understood from the photograph now laid before the Society."

I am sorry to say that the hope here expressed has not hitherto been realised. Year after year passed producing only more or less of radical reform in various mechanical details of the governor and of the fine movement, until about six months ago, when, for the first time, I had all except the pendulums in approximately satisfactory condition. By that time I had discovered that my choice of zinc and platinum for the temperature compensation, and lead for the weight of the pendulums, was a mistake. I had fallen into it about ten years ago through being informed that in Russia the gridiron pendulum had been reverted to because of the difficulty of getting equality of temperature throughout the length of the pendulum; and without stopping to perceive that the right way to deal with this difficulty was to face it and take means of securing practical equality of temperature throughout the length of the pendulum (which it is obvious may be done by simple enough appliances), I devised a pendulum in which the compensation is produced by a stiff tube of zinc and a platinum wire placed nearly parallel each to the other throughout the length of the pendulum. The two pendulums of the clock shown to the British Association were constructed on this plan. Now it is clear that the materials chosen for compensation should, of all those not otherwise objectionable, be those of greatest and of least expansibility. Therefore, certainly, glass or platinum ought to be one of the materials, and the steel of the ordinary astronomical mercury pendulum is a mistake. Mercury ought to be the other (its cubic expansion being six times the linear expansion of zinc) unless the capillary uncertainty of the mercury surface lead to irregular changes in the rate of the pendulum. The weight of the pendulum ought to be of material of the greatest specific gravity attainable; at all events unless the whole is to be mounted in an air-tight case; because one of the chief errors of the best existing pendulums is that depending on the variations of barometric pressure. The expense of platinum puts it out of the question for the weight of the pendulum, even although the use of mercury for the temperature compensation did not also give mercury for the weight. Thus even though as good compensation could be got by zinc and platinum as by any other means, mercury ought on account of its superior specific gravity (nearly three times that of lead) to be preferred to lead for the weight of the pendulum.

I have accordingly now made several pendulums (for tide-gauges) with no other material in the moving part than glass and mercury, and with rounded knife edges of agate for the fixed support; and I am on the point of making four more for two new clocks which I am having made on the plan which forms the subject of this communication. I have had no opportunity hitherto of testing the performance of any of these pendulums, but their action seems very promising of good results, and the only untoward circumstance which has hitherto appeared in connection with them has been breakages of the glass in two attempts to have one carried safely to Genoa for a tide-gauge made by Mr. White, to an order for the Italian Government.

As to the accuracy of my new clock, it is enough to look at the pendulum vibrating with perfect steadiness, from month to month, through a range of half a centimetre on each side of its middle position, with its pallets only touched during $\frac{1}{300}$ of the time by the escapement-tooth, to feel certain that, if the best ordinary astronomical clock owes any of its irregularities to variations of range of its pendulum or to impulses and friction of its escapement-wheel, the new clock must, when tried with an equally good pendulum, prove more regular. I hope soon to have it tried with a better pendulum than that of any astronomical clock hitherto made, and if it then shows irregularities amounting to $\frac{1}{10}$ of those of the best astronomical clocks, the next step must be to inclose it in an air-tight case kept at constant temperature, day and night, summer and winter.

ON THE TROPICAL FORESTS OF HAMPSHIRE[1]

ENGLAND at the present time has a climate far from tropical, but at the time to which this lecture refers the palm and spice plants flourished here; and hence the climate then may rightly be spoken of as actually tropical.

The data on which this inference is based are the fossil leaves which are found in the clays of the south of Hampshire. Out of the many thousands of such leaves obtained by me during summer holidays for many years past, some selected specimens were exhibited in a cabinet in the Loan Collection of Scientific Instruments. Other collections of leaves from this spot and from Alum Bay have been made, and may be seen in the British Museum. It is the district immediately along the line east and west of Bournemouth which has been specially examined, and it is in the lower Bagshot beds, which are, comparatively speaking, amongst the youngest of the geological scale, that the leaves referred to have been found.

These Bagshot beds need not detain us; but as I have referred to them as amongst the youngest in the geological scale, I may mention that above them we have the Bracklesham beds, full of marine forms; the Barton beds, also full of marine forms, but telling a tale of a different sea; the Headon, Bembridge, and Hempstead series, with many repetitions of marine and fresh-water conditions, indicating long lapses of time. There is, too, the whole Miocene period, of which we have no trace in this district, but which we believe from continental evidence was of vast duration. Then, too, there followed periods of immense length, during which England underwent its latest glacial epoch; after that, the time during which the gravels were formed. While, therefore, we speak of these beds as almost the youngest of our series, they belong to periods of an incalculably remote past.

It is from the cliffs principally, and from the deep cuttings of the recently constructed railway from Bournemouth to Parkstone, that our knowledge is mainly derived. There are, in addition, the diggings carried on

[1] Lecture in connection with the Loan Collection of Scientific Apparatus, given at the South Kensington Museum, December 2 1876, by J. Starkie Gardner, F.G.S.

for commercial purposes. Great interest attaches to these somewhat monotonous-looking cliffs, as it is from them that has been unearthed the marvellously rich flora which I shall briefly describe further on. Let us commence by visiting the diggings near Wareham. We see that they are situated on a wild moorland with hillocks, under the high range of chalk downs, in a gap in which stand the massive ruins of Corfe Castle. Very bleak and barren the scenery looks in high winds and driving showers, and the latter are of unusually common occurrence, the clouds being caught and held by the high downs. The moorland stretches far to the sea and enwraps Poole Harbour, continuing as far as the eye can reach, beyond Bournemouth to the tower of the fine old abbey of Christchurch and to the New Forest, being here and there clothed with extensive pine plantations. But in fine weather it has a charm of its own and is especially lovely when the yellow furze and purple heather are in full bloom. Even the actual diggings themselves are most picturesque, especially those now abandoned, as there we find little deep blue lakes surrounded by many-coloured cliffs fifty feet in height, in which bright yellows, magentas, and crimsons predominate. I do not apply the word blue to the small lakes poetically, for they are of an intense blue, finer in colour than the famed blue of the waters of some of the Swiss lakes. The heath is, in some patches, of a magenta colour, where a crimson clay patch forms the soil. If we examine these cliffs and banks we find them composed of clays dark or white, or red and white mottled, of layers of coarse grey grit and of sands of every shade of red and yellow, white, and variegated. Often the sands have angular lumps of clay imbedded in them. The quarrying is mostly done in open pits, the clay being dug out perpendicularly with a long and narrow spade. Some of the deeper seams are mined, and a considerable depth is reached in Mr. Pike's workings, and at Branksea it is worked under the sea-level. These pipeclays are exported to all parts of the world wherever good pottery is made.

Overlying the pipeclays we find another series of deposits, which are not here quarried for use, but looked upon as refuse; but near Bournemouth they are dug into in many places for the brick earth contained in them. They are easily distinguished by the darker colour and more sandy nature of the clays. These drab clay basins are of smaller extent and are full of remains of decayed leaves, and have actual seams of coal in them, which is burnt by the villagers. We now cross Poole Harbour, at high tide a magnificent sheet of water, the distant hills, behind which the sun sets, giving it the appearance of an Italian lake, and glance at Branksea Island as we pass—the owner of which, however, will not allow us to land. In the sheltered bay of Studland we can see but little of the cliffs, as they are now mostly overgrown to the very beach. We are struck, however, by the coloured sands which forcibly remind those of us who are familiar with them of the still more brilliant hues of the sands at Alum Bay.

Being ferried across the inlet of Poole Harbour and walking along the beach towards Bournemouth, we find the coast for the first mile composed of hills of blown sand, beyond which the cliffs we have been viewing from a distance rapidly rise. These cliffs are themselves of rather monotonous appearance, being devoid of the brilliant colouring so conspicuous at Alum and Studland Bays, but they are crowned for the greater part of their length with pine woods. Their colour varies from buff to white and from white to slate colour. We notice apparently endless successions of clays, sands, and grits deposited at different angles and without any single bed being traceable for more than a few yards. The cliffs, preserving the same characters for a distance of four miles, extend to near Boscombe, where we notice a change in their composition. The clays are black and still more sandy, the upper parts of the cliffs are far less steep and seem composed of loose white sands and shingle with a thick capping of gravel.

At length still further east these beds disappear beneath the sea in consequence of the general dip of the strata. The sand beds which follow, where they cap the cliffs, are recognised from a great distance by their greater slope from the cliff shorewards, for they are so loosely composed that every wind blows the sand away in clouds and leaves the shingle to rattle down on to the beach. So loose is this material that that part of the coast line which had cliffs composed of this sand has now but an insignificant height; all the sand has been blown away by wind and wasted by rain, until the shingle has been left dropping lower and lower, and the stones which neither wind nor rain could affect, have come closer and closer together. This is the cause of the land connecting Hengistbury Head being much lower than any other in the neighbourhood. The shingly beds are ancient sea beaches, and the slope of them to the ancient sea can still be seen in places. So long have they been exposed that the flint pebbles in them are sometimes almost decomposed, the familiar white coating to the flints being an inch or more thick. This shingle, which is composed of rounded pebbles, that tell the tale of a long rolling on the old sea beach, is now the source of the pebbles on the present beach, and the round condition of the pebbles on the present beach on this part of the coast is not as on the shore further towards Poole, or as at Brighton, the result of present wave action, although the existing sea has undoubtedly reduced the pebbles in size. They cannot be confounded with the later angular river gravels which everywhere cover this area.

At the peninsula of Hengistbury Head, about six miles beyond Bournemouth, the cliffs again rise, being at first composed of black, chocolate-coloured, and white sands with pebbles, and farther on of green clayey sands containing nodules of large irregularly-shaped concretions of sandy, argillaceous ironstone disposed in layers, until lately worked for iron and shipped to the smelting furnaces of South Wales. Beyond Christchurch Harbour we have cliffs of white sand which, according to my views, close the series.

Inland the country has a barren appearance except in the plantations, and the scattered brick pits afford no additional information of use to us in our present researches. There is but little of interest to the tourist except on the very edges of the district where the archæologist will be interested in the Minsters of Christchurch, with its associated ruins, Wimborne, and the ruins of Corfe Castle.

No order of arrangement is at first apparent in these beds, but by going backwards and forwards over the ground attentively there is, it seems to me, a very well-marked and recognisable sequence. I will now tell you what I take to be this sequence. It has never been submitted to geologists before, and it is possible, as is often the case with new work, that there may be some objections raised to it.

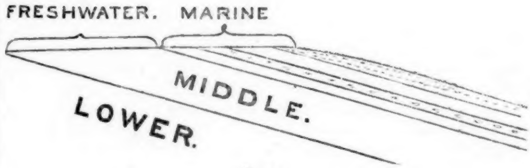


FIG. 1.

I would refer to the diagram (Fig. 1) where I have expressed my reading of this district. This lower fresh-water series is seen in the neighbourhood of Corfe and forms part of the cliffs at Studland. It is characterised by abundance of pipeclays, and has a thickness of 200 feet or more.

The middle freshwater series, also met with near Corfe

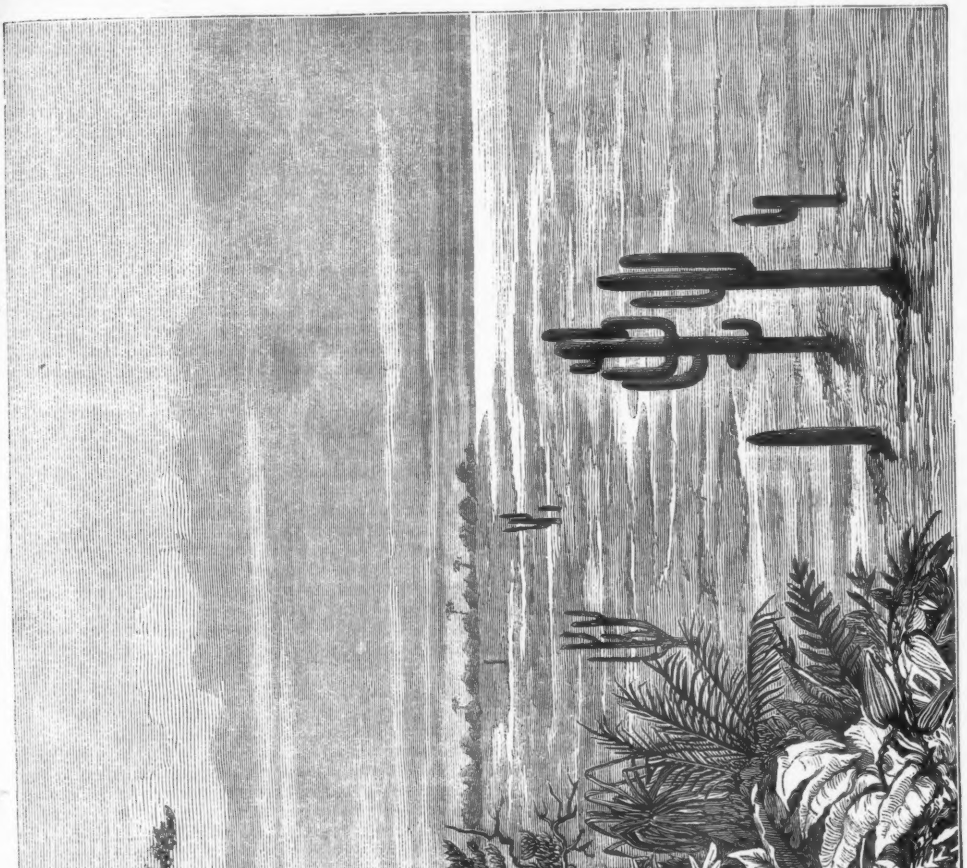


FIG. 2.—The Valley of the Bourne restored to represent the conditions which are supposed to have existed during the deposition of the Lower Bagshot Formation From a drawing used in illustration of Mr. Gardner's Lecture.

and at Studland, forms the whole thickness of the cliffs between Poole Harbour and Bournemouth. We thus have a magnificent section four miles long and 100 feet in height. Branksea Island is also formed of this series. Their entire thickness cannot yet be accurately stated, but may be put down at some 300 feet. They are characterised by the fact that the clays contained in them are usually brick-earth.

The next series above is a marine series and is some 400 or 500 feet thick. The base beds are dark sands and clays, succeeded by pebble beds and sands, then more sandy clays with pebbles, and ending with a thick deposit of white sands. This marine portion of the series occupies the cliffs between Boscombe and High Cliff.

Plain as this order of deposition appears, we have collateral proof that this interpretation is right, for at Alum Bay there is a complete section of the whole of these beds although somewhat thinned out, upheaved vertically, that is, turned completely on end, so that we can examine them in detail in the space of a few hundred yards, like passing in review volumes on a book-shelf. We see in succession the lower pipe-clays, the brilliant sands, the darker clays, sands, pebble-beds, one after the other, so tilted up and so placed that those who know nothing of the depression and elevation of areas can with difficulty be brought to believe that they have all been deposited horizontally.

In giving you the history of the deposition of these beds, I shall have to speak of a sinking area, and before doing so let me remind you that in Lyell's "Geology," a book in everyone's hands, many instances are recorded, of sinking areas in historic times, from our knowledge of which we feel justified in supposing that there were sinking areas in geological times.

The thick pipeclays and quartzose grits which we find at the bottom of the series can without the slightest hesitation be referred to the result of the wearing away of granite rock, for wherever granite is worn away by water, there we find white clays and similar quartz grits. We need not go further than Cornwall to see still finer clays, which have been produced in quite recent times from granite by the agency of water. The beds of this district included in the Tertiaries, first laid down over the chalk, were those now called London clay (a marine deposit), and when the streams which brought down our Bagshot beds first spread out their deposits, they spread over the London clay, except, perhaps, in those places where they first cut away the London clay, so that some of these Bagshots were possibly laid down on the chalk. The water in this case came from the west, and as here we are nearer the hills, which were the source of the clay, we find the grits coarser and the clays thicker.

At Studland the grits are not so coarse, and at Alum Bay, a long way east, the sands are very fine, so that anyone knowing the district could tell which of these specimens came from either place.

Each clay-patch represents a small lake, first scooped by the running water out of the beds just previously deposited, and then filled in by sediment. The mode of action is this:—The weather disintegrates the exposed surfaces of the distant granite rocks, and the loosened particles are carried by rain into streamlets, which convey them on to the river. The river, tearing and tumbling along, grinds the rocks which have fallen into its bed into round boulders, until in flood times the water is white with finely-divided granite. This grit being hurried along by the rush, is spread far and wide over the valley whenever the stream bursts its banks, which mountain torrents very often do, while the finer particles are still held and carried on until a lake or pool is met with, where the speed is checked; these fine particles are then dropped, and the water becomes quite clear. This deposition of fine clay goes on for ages, until the lake becomes filled up, the water gets diverted into another channel, and what was a lake becomes dry land; the river at the next flood spreads over the valley, covers in common with the surrounding ground what was the lake again and again with thick grits. Such is the origin of the large basins containing the clays which serve now to make your pipes and your crockery. You have only to recognise that the valley in which this takes place was slowly sinking, and there is no limit to the thickness of sands and clays which might be thrown down on any one spot, and in this way can be explained the sudden changes from grit to clay, which would else be a puzzle to us. The size of these old lakes is very well seen now wherever a clay basin has been quarried away, for the clay is quarried away for use whilst the sand is left. Some of them are represented by the beautiful blue pools I told you about, and are seen, therefore, to have been about one-quarter to one-third of a mile round, whilst their depths have varied from 30 to 60 feet. Mr. Lawrence Pike informs me that other clay basins are of larger extent, being $\frac{3}{4}$ of a mile in diameter. Their greatest length is in the direction of the valley. These clays extend under the surface, eastward, for they are worked at Branksea under the sea-level, at Parkstone, and near Bourne. At Alum Bay they are tilted up, and are full of beautiful fossil leaves.

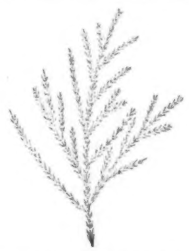
This next series of beds above, which I have told you are of a different character, mark a great change in the conditions of the land. The clay patches are of smaller extent, being the filling in of mere ponds or puddles, which acted on a smaller scale, as the lakes of which we have just spoken. The change indicated by these beds is one from the valley in which the previous contained beds were deposited, to a broad low-lying tract in proximity to the sea. We infer that we can trace how this tract became gradually lowered and lowered down to the sea-level.

The belief in the gradual lowering of the land in this area is borne out by the fact that in the cliffs near Poole, which are slightly lower in position than those farther east, we get only leaves of evergreens and forest trees, whilst as we work our way east so as to meet with beds on a higher level or, which is the same thing, of more recent age, we get a mixture of ferns and other plants, which require much moisture, whilst farther east still we get assemblages of plants that could only have lived in absolute swamps.

Low as the land appears to have become we have no evidence whatever, throughout the whole thickness of this part of the series, amounting to 300 feet at least, with an exception which I will tell you about directly, that it was low enough to be inundated by the sea, as the few shells that have been found are of fresh-water kinds. The exception alluded to is the occurrence of logs of wood bored by the ship-worm or teredo. All the ship-worms generally known to us live only in salt water, and are so delicately organised that the slightest mixture of fresh water instantly kills them. This isolated fact for some time presented a grave difficulty, but happening to read Mr. Gwyn Jeffreys' interesting account of the habits of this creature, I not only found that he relates the occurrence of similarly bored wood 300 miles up the River Gambia, but distinctly states that there is a species which lives in fresh water. Therefore this supposed marine indication may be on his authority removed, and, supposing this theory should be verified and universally accepted, we may safely infer that these middle beds are of fresh-water origin.

We now come to the third series of beds. A still continued sinking of the area brought this swampy condition so low that the sea was no longer kept out, but, bursting through, formed great salt-water lagoons teeming with life; for we suddenly find crowds of marine forms imbedded in what was formerly black mud, such as we might find now in the existing Poole Harbour here.

In this series of marine beds we have at the bottom lagoon beds, as I call them, which may represent a similar state of things to what we see at Christchurch, or Poole, or Weymouth, or any place where we have mud banks left dry or even shallow, between each returning tide. We still find here leaves of trees, many of them doubtless overhanging the lagoons, which have so slowly decayed, that they are overgrown with zoophytes; crowds of oysters are met with; we find the remains of shore-crabs, which from our knowledge of existing species, we infer, overran the muddy shore; the callianassa, a prawn-like creature, which bored through the mud; limpets, arcas, corbulas, and many other shell-bearing molluscs, passing their lives, dying, and becoming buried in the sediments of the sheltered lagoons. This lagoon condition went on until the gradual sinking has permitted the ever-encroaching surf to break over the lagoon barrier, to rush in, and in time overwhelm them with rolled shingle and sea-sand. We still trace the lagoon condition for a mile or so east, where it is represented by cigar-ash coloured sands, impregnated with salt, and coloured with this dark tint of carbonaceous matter. These sands contain very perfect remains of branches of a coniferous



BRANCH OF CONIFER

FIG. 3.—*Taxodium.*

tree resembling the genus Dacridium and large pieces of cactus. It should be mentioned that this is the earliest cactus known, and that the spines are found to be still flexible. The sands are in other places crowded with fruits something like those met with at Sheppey. Unfortunately the salt contained in them effloresces and splits all these specimens into fragments.

I may just tell you that at Hengistbury Head we have deeper sea deposits, with sharks' teeth and bones. At Highcliff, Barton, we have relics of a sea swarming with life, myriads of fossil shells may be collected on the cliffs, whilst still further on at Hordwell, we have beds showing that the land arose again, affording suitable conditions for the growth of luxuriant palms, and was the haunt of the alligator, turtle, and other reptiles which are now confined to tropical countries.

Fig. 2 is a view of the Valley of the Bourne at the time referred to above; a description will be given in the next article. (*To be continued.*)

GEOGRAPHICAL CURIOSITIES

DURING the meeting of the International Geographical Congress at Paris in 1875, the National Library opened an exhibition supplementary to that which was held in the Tuileries. Although very rich in documents and modern geographical works, the great national institution did not wish to show simply a duplicate of the collections exhibited at the Tuileries, and it therefore brought out only ancient and rare objects which the rules of the establishment wisely forbid to leave the building. Thus it showed to the public neither its great topographical maps, such as those of Cassini, van der Maelen, &c., nor its recent atlases, its numerous geological maps, its hydrographic charts of the French, English, and other Admiralty Departments. But, thanks to M. Leopold Delisle, Administrator-General of the National Library, and to M. E. Cortambert, Librarian of the Section of Maps and Plans, there was exhibited in the magnificent Mazarin Gallery a collection unique of its kind, and to which the Departments of Printed Books, Manuscripts, and Engravings contributed. The objects exhibited belonged generally to Group IV., devoted to Historical Geography and the History of Geography, and comprised, besides ancient and modern works and MSS. treating of geography and its history, ancient maps and globes, instruments used by ancient geographers, astrolabes, sundials, &c.

The success of the exhibition in the Mazarin Gallery inspired the Administration of the Library with the happy idea of transforming this temporary exhibition into a permanent institution. This has been established in the ground-floor of what is known as the "Salle des Globes," and in the two rooms which look out upon the great court of the Rue Richelieu, has been recently opened to the public who are admitted on Tuesdays from 10 to 4.

Although the limited space at disposal in these apartments has not permitted the transference of all the objects exhibited in the Mazarin Gallery, and although the Departments of Manuscripts and Printed Books have kept possession of some of the valuable documents lent on the occasion of the Geographical Congress, the exhibition is nevertheless of the greatest interest on account of the rarity of the objects which it contains. Space forbids us to give a complete list of the many objects exhibited, though we are able, through the courtesy of the editor of *La Nature*, to give illustrations and descriptions of a few of the curiosities. There are nearly 500 objects altogether, and those who desire a complete descriptive catalogue of them should procure No. 178 of the French journal just referred to.

On entering the first room of the exhibition the visitor is at once struck with the large dimensions of the two great globes of Coronelli, made, in 1683, by order of the Cardinal D'Estrees, who presented them to King Louis XIV. One of the most curious objects shown in this room is a map of the world, probably of the ninth or tenth century. It is a copy of one which appeared in a Commentary on the Apocalypse written by Beatus, a benedictine of the monastery of Valcovado in Leon, who lived in the eighth century. The original of which the one exhibited (Fig. 1) is a copy, belongs to the library of Turin. It shows strikingly the wonderful notions which these old monks had of the universe, and especially of the earth in which they dreamed their uneventful lives away. Four winds, represented by the grotesque figures seated upon the skin or leathern bottles, and holding shells in their mouths, indicate not the four cardinal points, but the collateral points, where the sun rises and sets at the summer and winter solstices. The orientation of the map, as was for long the custom in the middle ages, places the east at the top, the west below, the north on the left, and the south on the right. A circular ocean, the old river Oceanos of Homer, surrounds the world. If we examine the interior of this strange *mappemonde*, Europe will be seen on the left, Africa on the right, and Asia at the top. The Mediterranean is represented by a very regular parallelogram, extending from east to west. A not less regular branch of this sea occupies the place of the Archipelago, the Black Sea and the Sea of Azov, and bounds Europe on the east, the north-east point of the continent being indicated by the words *Hic Caput Europe* (Europæ). Islands uniformly square are spread over the Mediterranean; we may recognise under strange names, Corcyra, Cyprus, Samos, Sicily, Corsica; the name *Tassis*, which may also be noticed, designates, no doubt, the City of Tarsus, which the author evidently regards as an island.

In the surrounding ocean appear other isl nds not less fantastical. On the east the island of Crisa and Algure

(for Argire), in allusion to the region of gold and silver of the ancients, in trans-Gangetic India; on the north-east the Island of Tulé, which recalls the famous Thulé; Britannia; then the Island of Scotia, which, however, is not Scotland, as many might be apt to think, but the original home of the Scots, Ireland; for it was not till about the twelfth century that the name was fairly transferred to North Britain.

The orography of Europe is shown, partly in enormous cones, partly in elongated masses, five principal chains, of which only one is named, the Mountains of Gaul (*Montes Galliarum*), without doubt, the Pyrenees. The hydrography is wretchedly meagre. The largest river is correctly set down as the Danube (Danubu), but what a curious course is given to it! The second in extent is the Tagus, under the name of *Tavus*, which in utter contempt of geography, discharges itself into the Mediterranean. What considerable river is that which flows towards the east under the name of *Eusis*, a name still applied in Asia to a large river situated almost opposite to this one? Perhaps it may be meant for the Pontus Euxinus itself, the Black Sea; for to mistake a sea for a river was not an uncommon thing with these old geographers.

The political geography is of a higher kind than the physical geography. To speak only of Gaul we find mention made of Aquitaine, Toulouse, Gallia Lugdunensis,

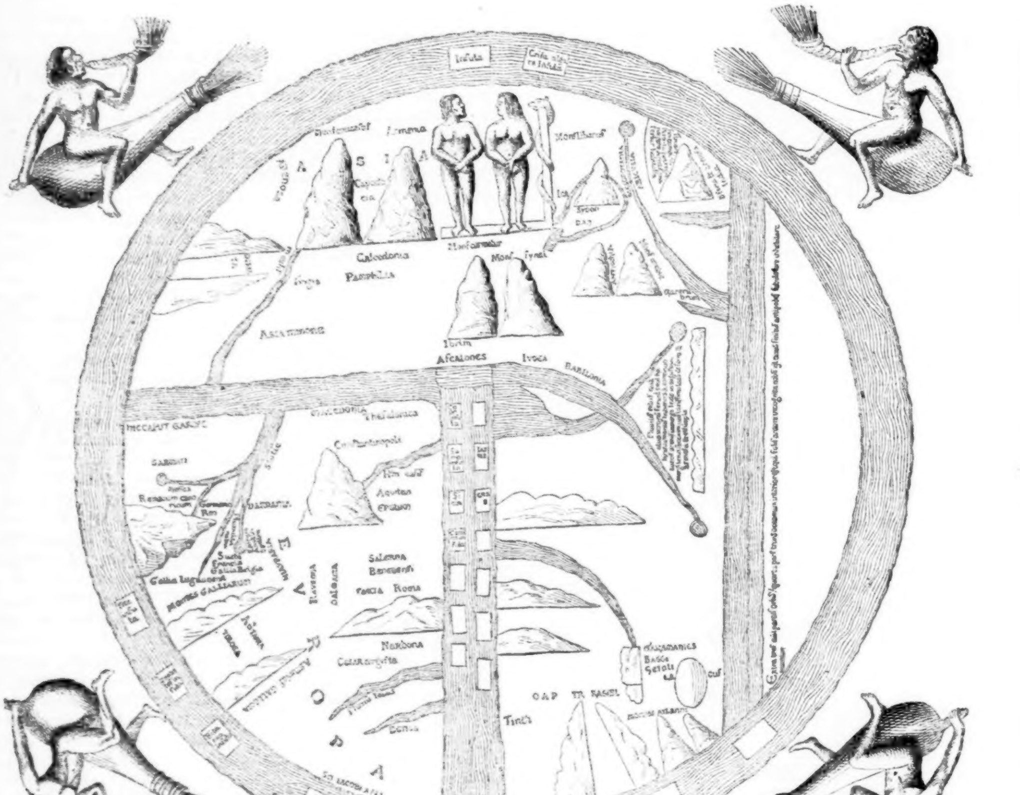


Fig. 1.—Map of the world contained in Beatus's Commentary on the Apocalypse (10th c.).

and Gallia Belgia; we also find *Francia*, which, however, does not stand for France but for Franconia.

In Asia, on the spot, no doubt, where Paradise was supposed to have been placed, appear Adam and Eve, in a grotesque position, and near them the serpent, who, however, has nothing tempting about him. Ten conical mountains surround this curious scene, without names except Libanus, the Caucasus, Carmel, and Sinai. Only three rivers flow in this vast space: first the Jordan, which encompasses Mount Lebanon in a very strange way; the Euphrates, which, though it bears no name, may be divined by the name Mesopotamia written on its banks; and then the Eusis, that mysterious Eusis to which we referred above.

The countries and the towns are more abundantly treated, though scattered pretty much at hazard. Jerusalem holds the first place, under the abbreviation, Ihrlm; Judea, Ascalon, Sidon, Antioch, Asia Minor, Phrygia, Mesopotamia, &c., are represented in situations more or less inexact.

In Africa, what strikes one at first is the Nile, the enormous Nile, divided near its sources into two branches, each issuing from a lake; it falls into the sea by a mouth larger than that of the Mediterranean itself. A note in-

serted between its sources tells of the gold which is mixed with the sand of the river, a vast lake which it traverses, and the sandy deserts of Ethiopia, through which it flows. The only other river seen in Africa is one without a name, which descends from the country of the Garamantes and falls into the Mediterranean; it is probably the Bagradas—the Medjerda of the present day. The mountains of Africa are but poorly shown. After Mount Atlas (*Montes Atlanni*), which is neither in its place nor very markedly brought out, there may be noticed three mountains which abut in the Mediterranean, and two steep and sharp-pointed mountains designated by the scarcely legible

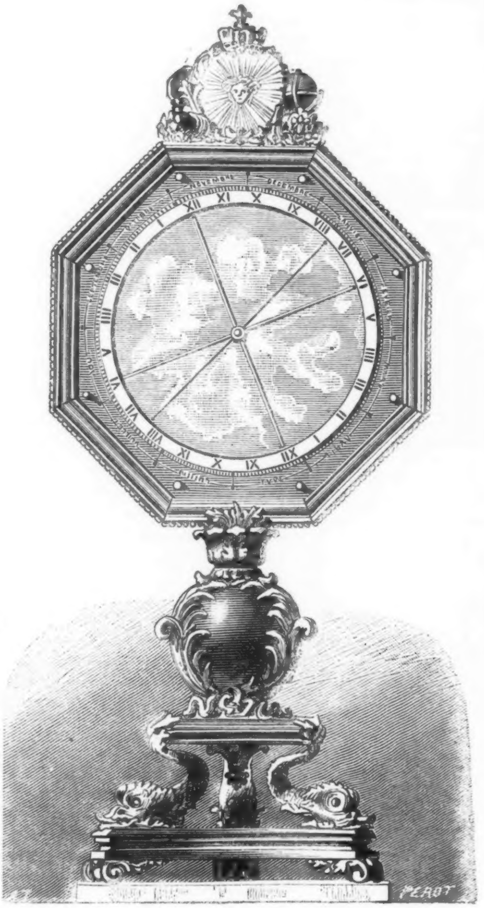


FIG. 2.—Copper cosmographic apparatus.

words, *duo Alpes Contra Arasibi;* this should perhaps be read *Contraria sibi, i.e.,* two mountains opposite each other, forming, as it were, two walls between which is a narrow passage. But where exactly are they?

A note inserted in the south of the map tells us that, independently of the three points of the known world, there is beyond the ocean a fourth part which is unknown to us on account of the heat of the sun, and on the confines of which, it is fabled, adds the author, that there are Antipodes.[1]

A reproduction of this map belonging to the 11th century shows a marked progress on that which we have just described.

FIG. 3.—Arab zodiac (one-fifth size of original).

The room in which this map is exhibited contains many equally curious objects, some of them of great rarity and

FIG. 4.—French astrolabe (one-fifth size of original).

value. Among these we may mention a copper cosmographic apparatus (Fig. 2) by Thuret, of date 1725. On

MARTIN BEHAIM'S MAP OF THE WORLD.

1492.

FIG. 5.—Martin Behaim's map ot the world.

one face are represented the northern constellations and the signs of the zodiac, as well as the correspondence of

[1] For the above details we are mainly indebted to an article recently pubshed by M. E. Cortambert.

these signs with hours and days; on the other side is shown the movement of the planets around the sun. It is worked by means of a clockwork mechanism in the interior, and is made to indicate the days, months, and years. In other rooms are shown the curious Arab zodiac represented in Fig. 3, and a French astrolabe (Fig. 4) made by François Chassignet, and of date Rome, 1622. Another interesting object is a facsimile map of the world, painted on parchment by order of Henri II. (1547–1559). Some critics, among others M. D'Anezac, date it as far back as the time of François I. (1515–47). In Fig. 6 we have reproduced a portion of South America after this curious document.

Among other objects is a facsimile of the well-known

FIG. 6.—North-east region of South America (16th c.).

globe of Martin Behaim, of date 1492, the original of which is at Nuremberg. This globe is extremely curious, specially on account of the lacuna in the region where America should be, that continent not having been discovered when the globe was made. An idea of the contents of this globe may be obtained from the illustration, Fig. 5. There is also the only known copy of the celebrated map of the world of Sebastian Cabot, of date 1544. A valuable legend in Latin and Spanish informs us, among other things, that John and Sebastian Cabot landed on the new continent in 1494.

Such are a few of the objects exhibited in this extremely interesting and instructive collection. No doubt many of our readers will be glad to know of its existence, and

those who take an interest in the progress of geography will doubtless think with us that such an exhibition adds one more to the many attractions of Paris; now that the Loan Collection is closed, nothing at all approaching it exists in London.

TEMPERATURES AND OCEAN CURRENTS IN THE SOUTH PACIFIC

IN the *Annalen der Hydrographie und maritimen Meteorologie* (Jahrg. iv., 1876, Heft 6, p. 219), Herr von Schleinitz, a member of the recent expedition in the German corvette *Gazelle*, states his views on ocean temperatures and currents; these are somewhat different from those expressed by Sir C. Wyville Thomson (Proc. Roy. Soc., vol. xxiv.), which are based on the data obtained during the *Challenger* expedition. The *Gazelle*, after leaving Auckland (New Zealand), pursued a course almost due north as far as the Fiji Islands; thence she proceeded to the Samoan Islands, situated at a short distance north-east of Fiji. After a brief excursion to the Tonga group and back, the *Gazelle* (from long. 172° 18'·5 W., and lat. 14° 28'·1 S.) sailed some 2,500 nautical miles in a south-south-east direction (to long. 141° 11'·4 W., and lat. 45° 33'·6 S.), after which she took a due easterly, and later on, a south-easterly course, to Magellan's Straits (long. 80° 30'·3 W., lat. 51° 41'·6 S.). The observations of temperature on the long cruise between the Samoan Islands and the Magellan's Straits are of special interest, as the course taken by the *Gazelle* lies to the south of that pursued by the *Challenger*.

On the first part of the course described, which has a direction nearly coinciding with the meridian, eight series of observations of temperature were made. The bottom profile of this part shows a peculiar absence of elevations, which is all the more remarkable when compared with any similar profile of the same length in the Atlantic.

The conclusion arrived at by Herr von Schleinitz, and based on the results of his observations is, that in the Pacific the arctic deep-sea current crosses the equator in a southerly direction and meets the antarctic current only between lat. 30° and 36° S. This is just the reverse of what takes place in the Atlantic, as it seems highly probable from the observations of both the *Challenger* and the *Gazelle* expeditions, that in the Atlantic the antarctic deep-sea current passes the equator, running northward of the same to a considerable distance.

Herr von Schleinitz concludes from these latter observations, that if the antarctic deep current enters the North Atlantic, even as a current of limited breadth, it must nevertheless carry enormous quantities of water from the South Atlantic to the North Atlantic, as it is certain that the current has a depth of more than 1,000 fathoms on the average. He then asks the question, What becomes of this mass of water? There is no strong surface current in existence which carries it back to the South Atlantic; even the current caused by the south-east trade winds runs more towards the Gulf Stream than towards the Brazilian coast current. There seems only one hypothesis possible, viz., that a great part of the water flows through the Arctic Sea and Behring's Strait into the North Pacific, and that may be the cause of the preponderance of the arctic current of this ocean over its antarctic one.

The natural conclusion drawn from this is that the South Pacific, in order to complete the whole circle, gives a great part of its waters to the South Atlantic, and as a proof of this it might be pointed out that the ice limit does not approach the equator so much anywhere as it does in the South Atlantic.

The following facts may also be mentioned as in favour of the hypothesis of a certain regular circulation taking place in the manner described. A comparison of the air-isotherms as well as the sea-isotherms both of the Atlantic and Pacific Oceans shows that (1) the South Atlantic is colder than the North Atlantic; (2) the North Atlantic is warmer than the North Pacific; (3) the South Pacific is warmer than the South Atlantic.

The higher temperature of the North Atlantic Ocean has hitherto been generally explained by the influence of the Gulf Stream. But a similar current exists in the North Pacific, and yet this is colder. There is no doubt that the Gulf Stream has a warming effect on some European coasts, but it is very probable that considering its comparatively small breadth of about 100 nautical miles, and shallow depth of only 100 fathoms, the stream is far too insignificant to be able to exercise a perceptible influence upon the climate of the whole North Atlantic and of the coasts surrounding this ocean.

On the other hand it does not seem to have been sufficiently appreciated hitherto, that a very large part of the North Atlantic is filled by water, which has crossed the equator, even if at a considerable depth. However trifling the rise in the temperature of this water, as caused by the passage over the equator, may be, when compared to the general temperature of the South Atlantic, it is nevertheless a fact that there is an important amount of heat, which the South Atlantic loses and the North Atlantic gains, on account of the very large extension of the current. Nor can it be objected with regard to this, that the mean temperature of that mass of water is probably below the mean temperature of air in the North Atlantic, because there is no question of absolute heat, but only of difference of temperatures between the North and South Atlantic.

The excess of water in the North Atlantic, which is not carried back into the South Atlantic by the surface-currents, and which passes through the Arctic Ocean (where it loses the heat it possessed) into the North Pacific, causes a decrease of temperature in the latter, and, proceeding southward, *i.e.*, again crossing the equator and thus absorbing heat, produces an increase of temperature in the South Pacific. Finally, the South Pacific gives back to the South Atlantic a part of that water at a very low temperature, which originally flowed from the latter into the North Atlantic perceptibly heated, on account of its passage through the tropics.

This circulation, however, is not to be understood as if the lowest strata of all the oceans took part in it; on the contrary, there are doubtless only single currents in the lower strata which follow it, while others may flow in an opposite direction. Further observations will throw light on these hypotheses; those made up to the present are yet insufficient and at times even contradictory. At the same time it must not be overlooked that a constant exchange of water between the lower and upper strata, *i.e.*, currents flowing in a vertical direction, are proved to exist beyond doubt, particularly in certain zones.

In conclusion Herr von Schleinitz considers the oceanic system of currents to be evidently a very complicated and at present obscure one, upon which the observations made on board the *Challenger* and the *Gazelle* throw but a very faint light.

The second part of the course pursued by the *Gazelle*, as described above, did not differ sufficiently in latitude, and therefore could not furnish any data which would be useful or decisive on the subject in question. However, the observations which were made give results in complete accordance with the hypothesis referred to above.

ON THE MEANS OF PROTECTION IN FLOWERS AGAINST UNWELCOME VISITORS

THE phenomena relating to this subject, which have important bearings on the doctrine of selection, have recently been discussed by M. Kerner in an interesting monograph communicated to the *Festschrift* published on occasion of the twenty-fifth anniversary of the Zoo-

logico-Botanical Society in Vienna. The following is a brief outline of this paper:—

M. Kerner, first of all, thinks it unwarrantable to divide the characters found in plants into *physiological*, which bring their possessors a certain advantage, and *morphological*, which are of no advantage. While, no doubt, profitless and even disadvantageous formations occur in plants, it is yet certain that such individuals are soon extinguished and suppressed by others which bear advantageous characters. Most of the so-called morphological characters have rather a certain biological significance, and it is only from the lack of observations regarding them that the material for their comprehension is so defective.

Hitherto study has mostly been directed to the relations between the forms of flowers and those of the animals visiting them. M. Kerner gives an account of those manifold forms hitherto regarded as only of morphological significance, but the use of which is to guard flowers against uninvited guests and against all injurious influence, and attacks to which they may be exposed; these forms therefore are of essential biological value.

How numerous are these enemies and uninvited guests will appear from the following brief sketch:—First, there are the large grazing animals, such as the ruminants, solipedia, &c. Then there are snails, especially the voracious Helicidæ, which, indeed, are seldom found in the flowers, not because they despise them, but because they are kept from them by a group of stiff bristles and prickles underneath the flowers. The same holds for soft insects, especially many larvæ of caterpillars. The wingless aphides are, specially among soft insects, to be noted as unwelcome guests of flowers. They are found extremely seldom in flowers, being warded off by suitable means, but if they are carried into the flower they immediately force their proboscis into the sappy tissue. The insects with a firm chitinous skeleton, again, easily pass over the bristles and prickles; only their posterior feelers are sensitive to contact with sharp points. Among animals of this class those are injurious to flowers which, in consequence of their too small size do not, in passing through to the nectar at the bottom of the flower, brush against either the anthers or the stigma. They take away the nectar without effecting fertilisation. But even when the chitinous insects are of the proper size they are unwelcome to flowers if they are wingless, for in that case they are a comparatively long time in reaching the flower of another individual of the same species, and the pollen with which they are laden is exposed to so many hazards, that fertilisation by these insects is extremely improbable.

Now the means of protection against access of these numerous animals are very various, as we shall presently see.

We may first notice the protection afforded by the leaves, which produce the building materials of the flowers and are necessary to their growth. They afford protection through certain alkaloids and other compounds contained in the cell-gap, and also through a hard leather-like consistence and thorny processes by which a portion of the leaves are protected from injury by grazing animals.

The means of protection in the flowers consist, first of all, in the production of matters which are repugnant to some animals; such are alcohols, resins, and etheric oils, to which a number of the unwelcome guests have such a dislike that they will rather endure the sharpest hunger than eat these plants.

A second kind of protection consists in prevention of approach to the flowers by isolation of these with water, as is the case in the Bromeliaceæ. Generally the foliage leaves have funnel-like forms in which the atmospheric precipitates, rain and dew collect and so form an insurmountable barrier to the passage of creeping, wingless insects, while the access of the flying insects which affect fertilisation is not prevented. The water-plants are also defended against unwelcome guests which might otherwise creep to them; and it is very remarkable that in water plants with projecting flowers, other means of protection against creeping animals are wanting; they are only developed when the isolating layer of water, from some cause or other, disappears. Very instructive in this relation is the behaviour of *Polygonum amphibium*. To the flowers of the plants growing in water, creeping insects cannot come, the flowers being surrounded with water. When, however, the water has run off and the plant is on dry ground, there develop on the leaves and stalks gland-hairs, which secrete a sticky matter, rendering the flower-bearing axis all smeary, so that access is equally forbidden to the creeping insects. If, now, a plant of Polygonum bearing these gland-hairs be put in the water again, the trichome-tufts with their sticky material disappear, and the surface appears once more smooth and even.

Such a formation of sticky matters is developed in very many plants as a sure protection against unwelcome visitants. These sticky matters appear on the most different parts of plants, under the flower, and ward off especially creeping, but also unwelcome flying animals from the flowers. The variety of the glandular forms yielding sticky matter is very great, and their occurrence is very widespread.

While these sticky matters are effective against creeping animals which have a pretty firm chitinous coat, and especially against ants, they are ineffective against the soft creeping animals, *e.g.*, the snails, which secrete slime on the sticky parts of the plants, enabling them to pass over these. Against such enemies the plants are armed with the most various thorns, prickles, and sharp teeth, which mostly have their points directed downwards, but may have the most diverse positions and forms. Quite peculiarly interesting are those prickles and needles, which serve the purpose not so much of keeping off unwelcome visitants as of showing to the insects which visit the flowers the right way for effecting fertilisation; whereas if the same insects visited the plants and removed the nectar by another way, fertilisation would not be accomplished.

The means of protection thus far described are all on the path which the unwelcome guest must traverse if he would reach the flower. There are other means of defence, however, within the flower itself. These, indeed, cannot be regarded as absolute, for they may be overcome by unwelcome visitors. They consist of hair-like formations, which are united in large numbers, into grating-like groups, rendering access impossible to one animal, while to another, which is furnished with a longer, thin proboscis, or can drive with greater force against the grating, they yield the desired food. These soft hair formations, which have the most various modifications towards the end in question, also often serve to point the way by which welcome visitors may reach the nectar.

Where all the formations that have been mentioned are wanting, protection is still afforded by bends, enlargements, and collocations of particular parts of plants, which are so diverse that it is difficult to indicate them cursorily. In general they may be divided into two groups, one of which comprises those formations by which the nectar is completely covered, whereas in the other the entrance is merely narrowed so that an opening remains by which the animals may introduce their sucking organs. The most different parts of the flower share in these formations, producing a very great variety of forms.

A last means of protection of flowers is represented in those numerous cases in which the flowers open only in the evening, and thereby are guarded against the visit of insects which swarm during the day. Further, there is the diversion of injurious insects, due to the fact of the nectaries being sometimes situated in other parts of the

plants, and mostly in the foliage leaves, so that creeping insects satisfy here their need of food, and do not trouble themselves about reaching the flowers higher up, and thus these remain protected from their visits.

"From the foregoing obervations," says M. Kerner, "it will sufficiently appear that the relations of plant-form to that of animals living at the expense of plants are far more manifold than has hitherto been supposed, and that especially numerous formations in foliage-leaves and stem are so far of biological significance that by them protection is afforded to the flowers against the prejudicial visits of certain animals. Where the attacking animals are absent this defence is also, naturally, useless, and therefore all these formations are properly to be regarded as means of protection only for those plant-stocks which occur in their original region—in the region where the species to which they belong has arisen. In another place they are perhaps not means of defence; indeed they may even be of disadvantage, or their formation there is at least something superfluous, not in the economy of the plant, and as a matter of course, these disadvantageous, because not economically organised plants, when they come under conditions which are not in harmony with their form, are driven out of the field by competitors that are more advantageously organised.

"If, for example, a plant species comes, in course of its migrations, into a region in which it is exposed to other attacks, or if the external relations in the place where the species arose (and with which it was formerly in agreement) are altered, it may become more and more rare, and gradually quite die out. Among these changes of external relations, however, are to be understood not merely changes of climate; a not less important part is played by the changes which occur in the animal world in a particular region. Apart altogether from changes in the extent of distribution of animals, the animals vary as well as the plants, and individual varieties, which occur with new characters that are advantageous relatively to given external conditions, may become the starting point of new species. What is of advantage, however, to the animals which attack the plants, constitutes, as a rule, a disadvantage for the attacked plant, and it is therefore not only possible, but in course of time it has actually often happened that in consequence of the multiplication of an advantageously organised animal form in a certain region, some plants in this same region having their flowering function destroyed, and their formation of seeds hindered, have disappeared gradually from the scene.

"While, on the one hand, the dying out of certain species with altered external relations, is at once explained by changes in the attacks of animals, the same relations, on the other hand, afford an explanation of the phenomenon, that under similar external conditions, plant species, which, with reference to other characters, are classed under the most different genera and families, do yet in certain formations agree with each other. Only the advantageous forms can maintain themselves, and only those individual varieties which appear with characters that are advantageous with reference to the conditions presented by the locality and position become the starting-points of new species. Since, however, the creation of new species in this way may occur in the most different plant-families, it is explicable that we find, *e.g.*, in one floral region, very many species of the most different stocks guarded with prickles, in another floral region such species furnished pre-eminently with flowers very rich in nectar, and that often even the character of the whole vegetation is determined by the preponderance of plants with like formations. Owing to the fact that the variety of the means of protection, as well as of the means of attraction is very great, and that through formations of the most different kind the same result can be reached, this conformity is again, of course, greatly limited. Indeed, precisely by this circumstance that, against the same prejudicial attacks, very different formations may serve as equally good means of defence, is the phenomenon explained that frequently several species of a family occur beside one another, without entering into competition in this relation, because the species, each after its own fashion, possess equal advantages."

THE ACTION OF THE WINDS IN DETERMINING THE FORM OF THE EARTH[1]

In view of the most recent discoveries in the region of physics, especially with regard to the nature and properties of forces, it became necessary *eo ipso* for dynamical geology to give up as unsatisfactory the division of geological forces into "igneous" and "aqueous," and to substitute a division of them into "primary" and "secondary"; of which the former explain all the motions which we observe on and in the earth, according to their origin and nature; while the others—one might call them "agencies" to distinguish them from the first—would teach us what and how great changes in the figure of the earth's surface are produced by the bodies so moved, through reciprocal action on each other. Sensible of this inevitable reform in dynamic geology, the author of an essay entitled "The Action of the Winds on the Configuration of the Earth," sought to call attention to the gaps hitherto existing in physical geography, and especially to show what a mighty and yet hitherto very little observed agent the wind is, considered as one of these secondary geological forces. In the following paper the author offers to the readers of NATURE a *résumé* of his memoir.

It is at once evident and conformable to nature that the winds are to be regarded, in the first instance, as a proof of the unequal insolation at different points of the earth's surface, but, in their direction and variation, they are immediately influenced now by the position of the sun, now by the earth's rotation and the distribution of the solid and the liquid; that the winds are, on the one hand, a product of these geophysical actions, and, on the other, become a special factor, of which not only the meteorologist, but also, in front rank, the geologist, is called on to take account. Since, that is to say, it is purely the winds which determine the condition of moisture of the atmosphere, and have to perform the *rôle* of distribution of rain over the entire surface of the earth, but at the same time, in their constant circulation from the equator to the poles and from the poles to the equator, represent an imposing motive force, it is obvious that to be able to prove and establish more fully their geological *rôle*, one must consider them in this twofold relation; on the one hand as a climatic-meteoric, on the other as a mechanical agent. Accordingly the essay referred to treats, in its first part, of the climatic-meteoric, in the second, of the mechanical action of the winds; while the third part comprehends those actions of the winds which they perform indirectly either in meteorological or in mechanical relation.

More particularly the *First Part* is concerned with the characteristics of the two principal wind systems, the polar and equatorial currents, and with their reaction on those continents and mountain-chains, by which, in their typical course—as is manifest on oceans and neighbouring coasts, especially west coasts, of continents—they are variously disturbed. The equatorial currents here appear as properly the distributors of precipitation, and therefore as the principal factors by which the transporting power of flowing water, or generally the levelling action of water on the earth's surface, is produced. The polar currents, on the other hand, discover a tendency to act contrary to the work of the equatorial currents, that is, to restore the precipitated water in vapour form to the atmosphere, and generally to further evaporation. In view, however, of the fact that not all the water, which by action of the winds is precipitated on the solid land, returns to the ocean or the atmosphere, these two air-currents together appear to be similarly empowered to empty entirely, some time, the immense water-basin of the earth from which they continually procure anew their freight of water, and meanwhile to continuously lower the sea-level, through by a very small quantity, and therefore to take a prominent part in the so-called secular elevation of continents.

These two air-currents, indeed, are not everywhere and always true to the character just given. On the contrary, when they have to accomplish a great work, and especially when a polar current has to rise over a lofty mountain, or an equatorial current

[1] Abstract, by Dr. Francis Czerny, of a memoir of his in the 48th supplementary number of Petermann's *Mittheilungen*.

has to traverse an extended surface of dry land, the former, as a rule, even appears as a rainy wind, while the latter, if its course have been *long* enough, appears as a dry wind; and if the mountain range be high enough, which the latter is required to rise over, this may, when it has reached the other side, even stream down as a hot, withering wind, the föhn of Continental writers, on the thirsty regions. If, then, we find in coast districts a greater yearly rainfall than further inwards, or if, on the wind side of the hills, we find lower snow-lines and further-reaching terminal moraines of the glaciers than on the lee side; or lastly, if we find successively regions of forests, of steppes, and of wastes, we may easily recognise therein each time an expression of the power of the winds, which, according as they are abundantly or poorly laden with aqueous vapour, or even quite dry, call forth this variety of geophysical phenomena.

The ever-moving atmosphere has therefore the most heterogeneous actions. While it feeds the glaciers and rivers, it causes at the same time a backward prolongation of these to the common source of the water—to the ocean, so as, with its moist breath, to produce everywhere simultaneously a formation of humus, and awake all into life; and again, where it is otherwise—where it appears as by nature a dry wind, or has been deprived of its freight of water, it brings with it drought and death. In the deserts the flora and fauna, then, have an extremely poor existence; the rivers no longer flow in a regular course; they have already been long in retreat, or are still only intermittent in their flow; the lakes also, when they are to be found in deserts, continually lose, through the constant evaporation, an abundance of water, although they may long have ceased to have an outlet to the neighbouring sea. In many deserts you do not meet with a single brook or pond; instead of such, you find only dried-up wadis and depressions, while the extensive stretches of waste, covered with salt incrustations and efflorescences, as also the scattered remains of dead animal species of past times, give evidence that these waste and withered regions formerly wore quite a different physiognomy; indeed, as the fossils and deposits of gypsum and salt testify, must even have been flooded with enormous inland seas. Now, probably (next to solar heat), it was above all, the winds, especially the dry winds, which, acting for a long period of time in the earth's history, dissolved these seas into aqueous vapour, carried them away, and so transformed the former lake bottom, now laid bare, into a waste.

Second Part.—But the winds are not only an expression for the general circulation of the air and aqueous vapour of the ocean; they are also a moving agent *quand même* not to be underestimated, since, in their progress, they communicate their own motion to all bodies which are not heavy enough to withstand them. Now, according as, in this way, solid or liquid bodies (especially the waters of the ocean) are put in motion, the mechanical action of the winds is to be considered from two distinct standpoints; first, its action on the solid land, and second, on the water, and through this again upon the solid land.

In the first case, it is the conditions of the strata that are continually altered under the action of the winds. It is at one time the snow, at another the salt-dust, at another the vegetable and animal remains, the ashes thrown out in volcanic eruptions, masses of dust and *débris*, or lastly, sand, that are whirled about, raised, carried miles away, and again deposited. The so-called wind-bedding characterises in every case the formations so produced, or in course of production. We shall cite here only two of the most remarkable examples of this kind of power in winds. One is the extensive Chinese Loess formation, which, according to von Richthofen's researches, appears to be quite a wind-formation; the other is the progressive dune-formation and dune-shifting on flat sandy coasts. Closer investigations (in this connection) of the conditions under which dunes are generally formed, examinations of their form, slope, and strike, and further, some materials furnished in the narratives of travel of Rohlfs and Dr. Tietze, have enabled the author to form a new theory as to the origin of sandy wastes, but especially to show that, as a rule, they are simply dune-formations on the shores of the former inland lake which has disappeared through evaporation. Some examples of sand scratches, sand cuttings, and the devastations wrought by hurricanes, illustrate the further mechanical action of the winds on the dry land.

Passing to the mechanical action of winds on water, we have, above all, to consider drift-currents (wind-drifts) and the motion of wind-waves. But while the former manifest themselves as powerful factors of transport, the wind-waves are besides characterised by a not unimportant effect in the direction of depth, but more especially by their now land-forming, now land-shattering surge. In this way the waters appear also as the most powerful medium through which the action of the winds is exerted on the solid land. In fact it is the capricious wind-wave that destroys and carries away whole stretches of coast in order to raise somewhere else and lay dry whole areas out of the oceanic depths (sand bars, sandbanks, flat coasts, delta formations, &c.). It is the wind-wave that ever renders active the principle of constant transformation in opposition to that of stability, and seeks to alter the contours of the dry land. Not without reason, then, does El. Reclus remark:—"It is by the movement of the atmosphere that we have to explain the form of continents." (*C'est par les mouvements de l'atmosphere, qu'il faut expliquer la forme des continents.*)

Third Part.—The winds, finally, produce, in an indirect way, many geological phenomena. According as they influence and determine the air-pressure, they cause now a perceptible swelling out, now a sinking of some large water surface, as has been observed on the oceans as well as on the North American and the Swiss lakes. Through this influence of air-pressure the winds further appear to affect volcanoes now favourably, now preventively—since also the lava masses in the crater (according to P. Scrope's representation) must be sensitive to atmospheric pressure; in high pressure finding a greater resistance, and in low pressure rising and breaking out more easily. The firedamp explosions also appear to be favoured by barometric depression. Similarly a certain connection can be demonstrated between the air-pressure and earthquakes.

Still more evidently are winds seen to exert an influence on earthquakes and volcanic phenomena, when regarded as rain-winds. With a large access of atmospheric water are connected both subterranean deluges and overturnings, and an abundant formation of steam in the heart of volcanoes; these circumstances immediately give rise to earth-tremblings, or violent volcanic outbursts and exhalations of steam.

If the hypothesis of T. R. Mayer, that the trade-winds are the principal cause of terrestrial magnetism, be correct, we must also, finally, ascribe to the winds an important part in the production of electricity. Lyon, Duveyrier, and Rohlfs have observed that the dry desert wind is uncommonly rich in electricity.

The author, indeed, has, in the course of his researches, given attention especially to the action of the winds in the most recent geological periods, and must in the meantime leave to specialists the more definite answering of the question how far the traces of action of winds also in the older periods of the earth's history can be followed. It is certain, however, that since in historical geology we have to do with a land flora and fauna, and with the (wrongly) so-called ocean precipitate, and so with the building up of sedimentary layers, we have so many undoubted proofs of the existence of rain and rivers, and accordingly of that of winds. Even when the earth was still a ball of glowing gas—and consequently a sort of sun to the moon's inhabitants, we can conceive the wind already acting as a geological agent—with the proviso, indeed, that the theory (of Faye and Reye) which regards the sun-spots as cyclone-like phenomena, or Secchi's view that the temperature of the sun at the sun's equator is higher than beyond the 30th degree of latitude, be verified.

UNDERGROUND TEMPERATURE[1]

A REMARKABLE series of observations have recently been taken in a boring at Sperenberg, near Berlin. The bore was carried to the depth of 4,052 Rhenish (or 4,172 English) feet, and was entirely in rock salt with the exception of the first 283 feet, which were in gypsum with some anhydrite. The observations were taken under the direction of Herr Eduard Dunker, of Halle-an-der-Saale, and are described by him in a paper occupying thirty-two closely printed quarto pages (206-238) of the *Zeitschrift für Berg-Hütten-und-Salinen-Wesen* (xx. Band, 2 und 3 Lieferung, Berlin, 1872).

[1] Ninth Report of the British Association Committee, consisting of Prof. Everett, Sir W Thomson, F.R.S, Prof. J. Clerk Maxwell, F.R.S., G. J. Symons, F.M.S., Prof. Ramsay, F.R.S., Prof. A. Geikie, F.R.S., James Glaisher, F.R.S., George Maw, F.G.S., W. Pengelly, F.R.S, Prof. Hull, F.R.S., Prof. Ansted, F.R.S., Prof. Prestwich, F.R.S., Dr. C. Le Neve Foster, Prof. A. S. Herschel, G. A. Lebour, F.G.S., and A. B. Wynne, appointed for the purpose of investigating the Rate of Increase of Underground Temperature downwards in various Localities of Dry Land, and under Water. Drawn up by Prof. Everett, Secretary.

The instrument employed for measuring the temperatures was the earth-thermometer of Magnus, which gives its indications by the overflowing of mercury, which takes place when the instrument is exposed to a higher temperature than that at which it was set. To take the reading, it is immersed in water a little colder than the temperature to be measured; the temperature of this water is noted by means of a normal thermometer, and at the same time the number of degrees that are empty in the earth-thermometer is noted. From these data the maximum temperature to which the instrument has been exposed can be deduced, subject to a correction for pressure, which is not very large, because the same pressure acts upon the interior as upon the exterior of the thermometer.

In the following *résumé* (as in the original paper) temperatures are expressed in the Réaumur scale, and depths in Rhenish feet, the Rhenish foot being 1·029722 English foot.

Observations were first taken at intervals not exceeding 100 feet, from the depth of 100 feet to that of 4042 feet, the temperature observed at the former depth being 11·0, and at the latter 38·5; but all these observations, though forming in themselves a smooth series, were afterwards rejected, on the ground that they were vitiated by circulation of water and consequent convection of heat.

It has often been supposed that though this source of error may affect the middle and upper parts of a bore, it cannot affect the bottom; but the Sperenberg observations seem to prove that no such exemption exists. When the bore had attained a depth of nearly 3,390 feet, with a diameter of 12 inches 2 lines at the bottom, an advance bore of only 6 inches' diameter was driven 17½ feet further. A thermometer was then lowered half-way down this advance bore, and a plug was driven into the mouth of this advance bore so as to isolate the water contained in it from the rest of the water above. After twenty-eight hours the plug was drawn and the thermometer showed a temperature of 36·6. On the following day the temperature was observed at the same depth without a plug, and found to be 33·6. Another observation with the plug was then taken, the thermometer (a fresh instrument) being left twenty-four hours in its position. It registered 36·5, and again, without plugging, it gave on the same day 33·9. It thus appears that the effect of convection was to render the temperature in the advance bore 3° R. too low.

Apparatus was then employed for isolating any portion of a bore by means of two plugs at a suitable distance apart with the thermometer between them. This operation was found much more difficult than that above described, but in several instances it gave results which were deemed quite satisfactory; while in other instances the apparatus broke, or the plugging was found imperfect. The deepest of the successful observations by this method was at 2,100 feet, and the shallowest was at 700 feet. The first 444 feet of the bore was lined with iron tubes, between which the water had the opportunity of circulating even when the innermost tube was plugged, hence the observations taken in this part were rejected.

All the successful observations are given in the third column of the following table, subject to a correction for pressure; and, for the sake of showing the error due to convection in the ordinary mode of observing, the temperatures observed at the same depths when no plugs were used, are given in the second column:—

Depth in feet.	Temperature Réaumur. Without plugging.	Temperature Réaumur. With plugging.	Difference.
700	16·08	17·06	0·98
900	17·18	18·5	1·32
1,100	19·08	20·8	0·72
1,300	20·38	21·1	0·72
1,500	22·08	22·8	0·72
1,700	22·9	24·1	1·2
1,900	24·8	25·8	1·0
2,100	26·8	27·1	0·3
3,390	34·1	36·15	2·05

These temperatures are not corrected for pressure, but they are corrected for rise of zero in the normal thermometer; and this last circumstance explains the difference of 0·4 between the temperature 36·15 here given and 36·55, which is the mean of the above-mentioned observations at the depth of 3,390 feet.

Another proof of the injurious effect of convection was obtained by comparing the observed temperatures (without plugging) in the first 400 feet of the great bore, designated Bore I., with the temperatures observed at the same depths during the sinking of another bore, designated Bore II., near it; the observations in this latter being always taken at the bottom. The following were the results:—

Depth Feet.	Temperatures. Bore I.	Temperatures. Bore II.
100	11·0	9·0
200	11·6	10·4
300	12·3	11·5
400	13·6	12·5

The temperature at the depth of 100 feet in the great bore thus appears to have been raised about 2° R. by convection.

The following is a table of the successful observations, corrected for pressure:—

Depth in Rhenish feet.	Temperature Réaumur.
700	17·275
900	18·780
1,100	21·147
1,300	21·510
1,500	23·277
1,700	24·741
1,900	26·504
2,100	28·668
3,390	37·238

Assuming, with Herr Dunker, the mean temperature of the surface to be 7·18, which is the mean annual temperature of the air at Berlin, we have the following increments of temperature with depth:—

Depths in Rhenish feet.	Increment of depth.	Increment of temperature.	Increase per 100 feet deg. Réau.	Increase per 100 feet deg. Fahr.
0 to 700	700	10·095	1·442	3·24
700 to 900	200	1·505	·752	1·69
900 to 1,100	200	2·367	1·184	2·66
1,100 to 1,300	200	0·363	·182	·41
1,300 to 1,500	200	1·767	·884	1·99
1,500 to 1,700	200	1·464	·732	1·65
1,700 to 1,900	200	1·763	·882	1·98
1,900 to 2,100	200	2·164	1·082	2·43
2,100 to 3,390	1,290	8·570	·664	1·49
0 to 3,390	3,390	30·058	·887	2·00

The mean rate of increase found by comparing the temperatures at the surface and 3,390 feet is exactly 1° Fahr. for 50 Rhenish or 51·5 English feet.

The numbers in the last two columns exhibit upon the whole a diminution with increase of depth; in other words, the temperature increases less rapidly as we go deeper down. As regards the first 700 feet, which exhibit a decidedly more rapid rate than the rest, it must be remembered that nearly half of this distance was in a different material from the rest of the bore, being in gypsum with some anhydrite, while all the rest was in rock salt. Prof. Herschel has found, in recent experiments not yet published, that the conductivity of rock salt is exceedingly high; and theory shows that the rates of increase, in superimposed strata, should be inversely as their conductivities. We may, therefore, fairly attribute the rapid increase in the first 700 feet

to the relatively small conductivity of the portion (283 feet) which is not rock salt. The slow rate of increase observed in the long interval between the depths of 2,100 and 3,390 feet is not so easily accounted for; we can only conjecture that this and the other inequalities which the above table presents, for depths exceeding 700 feet, are due to fissures or other inequalities in the rock which have not been put in evidence.

With the view of summing up his results in small compass, Herr Dunker has assumed the empirical formula—

$$t = 7\cdot18 + ax + bx^2,$$

t denoting the temperature (Réaumur) at the depth x (Rhenish feet); and has computed the most probable values of a and b, by the method of least squares. He finds

$$a = \cdot0129857 \qquad b = -\cdot000000125791,$$

the negative sign of b indicating that the increase of temperature becomes slower as the depth increases.

A paper by Prof. Mohr, of Bonn, as represented by an abstract published in NATURE (vol. xii. p. 545), has attracted attention from the boldness of its reasoning in reference to the Sperenberg observations. Prof. Mohr, however, does not quote the observations themselves, but only the temperatures calculated by the above formula, which he designates, in his original paper (*Neues Jahrbuch für Mineralogie*, &c., 1875, Heft 4), "the results deduced from the observations by the method of least squares." In the abstract in NATURE they are simply termed "the results of the thermometric investigation of the Sperenberg boring," a designation which is still more misleading.

Attention is called to the circumstance that the successive increments of temperature for successive equal increments of depth, form an exact arithmetical progression, as if this were a remarkable fact of observation, whereas it is merely the result of the particular mode of reduction which was adopted, being a mathematical consequence of the assumed formula $t = 7\cdot18 + ax + bx^2$. The method of least squares is not responsible for this formula, but merely serves, after this formula has been assumed for convenience, to give the best values of a and b.

Herr Dunker, in his own paper, lays no stress upon the formula, and gives a caution against extending it to depths much greater than those to which the observations extend. Writing to Prof. Everett under date April, 1876, he requests that in the summary of his results to be given in the present Report, the formula should either be suppressed or accompanied by the statement that its author reserves a different deduction.

The following are the differences between the temperatures computed by the formula and the observed temperatures:—

Depth.	Difference (computed *minus* observed).
700	− 1·621
900	− 0·931
1,100	− 1·204
1,300	+ 0·427
1,500	+ 0·553
1,700	+ 0·882
1,900	+ 0·811
2,100	+ 0·238
3,390	+ 0·482

The necessity of adopting some means to prevent the circulation of water in bores, has for some time been forcing itself upon the attention of your Committee. Many of the observations taken by their observers have contained such palpable evidence of convection as to render them manifestly useless for the purpose intended; and in the light of the Sperenberg experiments it is difficult to place much reliance on any observations taken in deep bores without plugging. The selection of a suitable form of plug is now occupying the careful attention of your Committee.

Herr Dunker's paper gives a very full account of the different kinds of plug employed at Sperenberg.

For stopping the mouth of the advance-bore the plug had a tapering shape, and was of hard wood, strengthened by two iron rings, one at each end, and covered with a layer of tow 5 lines thick, outside of which was thick and strong linen, nailed above and below to the wood, through a leather strap. It was lowered into its place by means of the iron rods used for boring; and, when in position, was pressed home by a portion of the weight of the rods. The plug carried the thermometer suspended from it. Its extraction was commenced by means of a screw on the beam of the boring machine, in order to avoid a sudden jerk, which might have broken the thermometer. The force which was found necessary for thus starting the plug, as well as the impression observed upon it when withdrawn, showed that it had fitted tight. To insure a good fit, the top of the advance-bore had been brought to a suitable shape, and its inequalities removed, by means of a revolving cutting-tool. Herr Dunker remarks that this plan is adapted to a soft material like rock-salt, but that in ordinary hard rock it would be better to make the bottom of the main bore flat, and to close the advance-bore by an elastic disc pressed over it. The method of observation by advance-bores can only be employed during the sinking of the bore, a time when it is difficult to avoid error arising from the heat generated in boring. The expense of making an advance-bore at each depth at which an observation is required is also an objection to its use.

Another kind of plug devised by Herr Dunker, and largely used in the observations, consisted of a bag of very stout india-rubber (9 millimetres thick) filled with water, and capable of being pressed between two wooden discs, one above and the other below it, so as to make it bulge out in the middle and fit tightly against the sides of the bore. On the suggestion of bore-inspector Zobel, the pressure was applied and removed by means of screwing. Two steel springs fastened to the upper disc, and appearing, in Herr Dunker's diagram, very like the two halves of a circular hoop distorted into an oval by pressing against its walls, prevented the upper disc from turning, but offered little resistance to its rising or falling. The lower disc, on the contrary, was permitted to turn. Both discs were carried by the iron boring-rods. Rotation of these in one direction screwed the discs nearer together, and rotation in the other direction brought them further apart. The india-rubber bag could thus be made to swell out and plug the bore when it was at the desired depth, and could be reduced to its original size for raising or lowering. In order to prevent the boring-rods from becoming unscrewed one from another, when rotated backwards, it was necessary to fasten them together by clamps, a rather tedious operation in working at great depths.

In taking observations at other points than the bottom, two of these plugs were employed, one above and the other below the thermometer.

In some of the experiments, the apparatus was modified by using linen bags filled with wet clay, instead of indian-rubber bags filled with water; and, instead of screwing, direct pressure was employed, the lower disk being supported by rods extending to the bottom of the bore, while the upper disk could be made to bear the whole or a portion of the weight of the rods above it. Some successful observations were obtained with both kinds of bag; but the water-bags were preferred, as returning more easily to their original size when the pressure was removed, and consequently being less liable to injury in extraction. In some observations since taken in another place (Sudenberg), Herr Dunker states (in the private letter above referred to) that indian-rubber bags, filled with water, and pressed, not by screwing, but by the weight of the rods, were employed with much satisfaction.

All the methods of plugging employed by Herr Dunker involved the use of the iron rods belonging to the boring apparatus, and therefore would be inapplicable (except at great expense) after the operation of boring is finished and the apparatus removed.

It seems desirable to contrive, if possible, some plug that can be let down and raised by a wire. In the first report of your Committee, it was suggested that two bags of sand, one above and the other below the thermometer, should be used for this purpose. Bags of sand, however, would be liable to rub off pieces from the sides of the bore, and thus to become jammed in drawing up. Mr. Lebour has devised a plug which will be of small diameter during the processes of lowering and raising, but can be rendered large and made to fit the bore, when at the proper depth, by letting down upon it a sliding weight suspended by a second wire. Sir W. Thomson suggests that a series of indian-rubber disks, at a considerable distance apart, will probably be found effectual.

Mr. Boot has continued his observations in the bore which he is making at Swinderby, near Scarle (Lincoln). It has now been carried to the depth of 2,000 feet, and is in earthy limestone or calcareous shale, of carboniferous age. Its diameter in the lower part is only $3\frac{1}{8}$ inches. In April last the temperature, 78° F., was observed at 1,950 feet; and more recently 79° F.

was observed at 2,000 feet; the water, in each case, having been undisturbed for a month. Supposing these results not to be vitiated by convection, and assuming the mean temperature at the surface to be 50°, we have an increase of 29° in 2,000 feet, which is at the rate of 1° in 69 feet.

Mr. Symons has taken a series of observations at the depth of 1,000 feet in the Kentish Town well, with the view of determining whether the temperature changes. The instrument employed is a very large and delicate Phillips' maximum thermometer. The following is a list of the observations:—

Date of lowering.	Depth indicated. Feet.	Thermometer set at.	Date of raising.	Depth indicated. Feet.	Temperature. Fahr.
1874 —	1000	64·50	May 8	1007	66·82
,, May 8	1000	63·80	July 2	1009	(reading) lost.
,, July 2	1000	63·20	July 28	1005	67·40
,, July 28	1000	65·10	Sept. 8	1004	67·51
,, Sept. 8	1000	65·80	Sept. 29	1004	67·43
,, Sept. 29	1000	65·81	Oct. 30	1006	67·68
,, Oct. 30	1000	63·40	Dec. 3	1006	67·52
,, Dec. 3	1000	63·80	Jan. 7	1009	67·63
1875 Jan. 7	1000	63·75	Feb. 1	1006	67·56
,, Feb. 1	1000	63·90	March 3	1005	67·58
,, March 3	1000	63·90	May 3	1006	67·62
,, May 3	1000	63·95	June 1	1005	67·49
,, June 1	1000	63·00	July 7	1005	67·53
,, July 7	1000	63·87	Aug. 3	1004	67·58
,, Aug. 3	1000	63·87	Sept. 10	1004	67·58
,, Sept. 10	1000	64·00	Oct. 2	1003	67·58
,, Oct. 2	1000	63·90	Oct. 19	1004	67·62
,, Oct. 19	1000	63·80	Nov. 1	1005	67·62
,, Nov. 1	1000	63·70	Dec. 1	Wire broke.	

The "depth indicated" is shown by a measuring wheel or pulley, over which the wire runs by which the thermometer is raised and lowered, as described, with a diagram, in the Report for 1869. The above table shows that there is always some stretching, real or apparent, in the interval between lowering the thermometer and raising it again. Recent observations by means of a fixed mark on the wire, have shown that the change is not, in the main, a permanent elongation, but an alternation of length. It is probably due in part to the greater tension which the wire is under in raising than in lowering, a circumstance which will cause a temporary difference of length variable with the rapidity of winding up; also in part to the circumstance that the wire is warmer when it has just left the water than when it is about to be let down. Some portion of the irregularity observed may be due to variations of temperature in that part of the well (210 feet) which contains air. The observations taken as a whole, show that any variations of temperature which occur in this well at the depth of 1,000 feet, are so small as to be comparable with the almost inevitable errors of observation. The observations will be continued at intervals of six months, with additional precautions, and with an excessively slow (specially constructed) non-registering thermometer, in addition to the maximum thermometer hitherto employed.

Through the kindness of the eminent geologist, M. Delesse, of the Ecole Normale at Paris, observations have been obtained from the coal-mines of Anzin, in the north of France. They were taken under the direction of M. Marsilly, chief engineer of these mines. Maximum thermometers of the protected Negretti pattern, were inserted in holes bored horizontally to the depth of ·6 or ·7 of a metre in the sides of shafts which were in process of sinking, and in which there was but little circulation of air. A quarter of an hour was allowed to elapse in each case, after the boring of the hole, before the thermometer was inserted, and the hole plugged. Four different shafts were tried. Those designated as Nos. I., II., III., were in the mine Chabaud La Tour; and No. IV. was in the mine Renard.

In Shaft I. observations were taken at eight different depths, commencing with the temperature 56¼° F. at the depth of 38·5 metres, and ending with 67¾° F. at 200·5 metres.

In Shaft II. there were observations at four depths, commencing with 55° at 87·3 m., and ending with 63¼° at 185 m.

In Shaft III. there were observations at three depths, commencing with 56° at 87·8 m., and ending with 62¼° at 144 m.

These three shafts, all belonging to the same mine, were very wet, and the temperature of the air in them was 11° or 12° C. (52° or 54° F.).

In Shaft IV., which was very dry and had an air temperature of about 15° C. (59° F.), observations were taken at 6 depths, commencing with 70¾° F. at 21·2 m., and ending with 84° F. at 134·8 m.

The mean rates of increase deduced from these observations are:—

In Shaft I., 1° F. in 14·4 m., or in 47·2 feet.
,, II., ,, 11·5 m., ,, 37·7 ,,
,, III., ,, 8·65 m., ,, 28·4 ,,
,, IV., ,, 8·57 m., ,, 28·1 ,,

The observer mentions that in Shaft II. there was at the depth of 90 m. a seam of coal in which heat was generated by oxydation; but no such remark is made with respect to any of the other shafts, although it is obvious that some disturbing cause has rendered the temperatures in Shaft IV. abnormally high. Possibly the heat generated in boring the holes for the thermometers in this shaft (which was dry) has vitiated the observations, the instruments employed being maximum thermometers. Two of the slow non-registering thermometers mentioned in last year's Report have been sent to M. Delesse, to be used for verification.

The slow-action thermometers are constructed on the following plan:—The bulb is cylindrical and very strong, and is surrounded by stearine or tallow, which fills up the space between it and a strong glass shield in which the thermometer is inclosed. The shield is not hermetically sealed (not being intended for protection against pressure), but is stopped at the bottom with a cork, so that the thermometer can be taken out and put in again if desired. Stearine and tallow were selected, after trials of several substances, including paraffin-wax, bees'-wax, glue, plaster of Paris, pounded glass, and cotton wool. The thermometers are inclosed in copper cases lined with india-rubber. When placed, without these cases, in water differing 10° from their own temperature, they take nearly half a minute to alter by one-tenth of a degree.

In concluding this Report, your Committee desire to express their regret at the losses which they have sustained by the deaths of Prof. Phillips, Sir Charles Lyell, and Col. Strange, of whose valuable services they have been deprived within the last three years.

OUR ASTRONOMICAL COLUMN

THE ROTATION OF SATURN ON HIS AXIS.—On December 7 Prof. Hall, of the Naval Observatory at Washington, observed a very white spot on the disc of Saturn, just below the ring. At 6h. 18m., Washington mean time, the spot was central, and it was watched from 22h. 40m. sidereal time, to 0h. 10m., as it moved across the disc. It was small, very well defined, and from 2″ to 3″ in diameter. A rough ephemeris of its motion was sent to various observers in the U.S., and the observations which have been received are as below. The second column gives the time when the spot was central.

	Washington. M.T. h. m.	Place.	Observer.	o – c
1876, Dec. 7	6 18	Washington	Hall	...
10	6 11	Cambridge	A. G. Clark ...	– 8m
—	6 3	Hartford	D. W. Edgecomb	0
—	6 4	Poughkeepsie	M. Mitchell ...	– 1
—	...	Albany	L. Boss	...
13	5 47	Washington	Hall	0
—	5 50	,,	Eastman	– 3
—	5 38	Cambridge	A. G. Clark ...	+ 9

The column o – c gives the residuals when a rotation-time of 10h. 15m. is assumed. At Albany, on December 10, the spot was seen, but not until after passing the centre.

[It will be remarked that the time of rotation, supported by the above observations of a very definite spot, is in close agreement with the result obtained by Sir W. Herschel from noting the successive appearances of a belt during the winter of 1793-94,

viz. 10h. 16m. 0·4s. Doubt has been occasionally expressed with regard to Sir W. Herschel's rotation-period from the uncertainty attaching to such observations, and the interesting confirmation of it just arrived at by the American observers will therefore be the more welcome. The Herschelian rotation for the planet globe of Saturn has been sometimes confounded with a rotation not depending upon observations, but calculated on Kepler's law for a satellite at an apparent mean distance equal to the semi-diameter of the middle of the ring; thus, Baily, in his "Astronomical Tables and Formulæ"—which were widely quoted for many years—has 10h. 29m. 17s. for time of rotation both of the globe and the ring.]

THE NEBULA IN THE PLEIADES.—Mr. Maxwell Hall, of Jamaica, communicates some observations of this nebula made on October 20, 1876, with a 4-inch Cooke equatorial, and power 55. "The nebula was 'bright,' according to Sir John Herschel's scale, and extended in a parabolical form at least 40′ from Merope, which was at the focus, while the axis of the figure was nearly S. of that star."

The difficulty of seeing with very large instruments a very faint nebulosity in close proximity to a bright star is strikingly illustrated by a remark made by Mr. Dreyer, observing with Lord Rosse; he states—"The Merope-nebula is never perceived with Lord Rosse's telescopes." So also D'Arrest sought for it in vain with the Copenhagen refractor, subsequently referring it to the class of which we are writing, which may be invisible in a great telescope but seen without difficulty in the finder. Vols. lviii. and lix. of *Astronomische Nachrichten* may be consulted for the earlier discussions as to the variability of this object.

VARIABLE STARS.—Prof. Schönfeld has published in *Vierteljahrsschrift der astronomischen Gesellschaft*, xi. Jahrgang, Heft 4, an ephemeris of the maxima and minima of most of the variable stars for 1877, including Algol, λ Tauri, S Cancri, δ Libræ, and U Coronæ Borealis, which have short periods. The max. of χ Cygni is dated February 6, and the min. on September 15; Mira Ceti, min. on July 23, max. on November 10.

Schmidt's star in Cygnus was red on January 7, and about equal in brightness to the star + 41° No. 4243 in the "Durchmusterung," but the difference of only 0·5m. between the catalogue brightness of this star and that of + 42° No. 4204, certainly did not represent their relative intensity of light on this evening. The variable might be estimated at 7·2m. by reference to the latter star.

METEORS OF JANUARY 7.—In the early part of this night a number of meteors were remarked near London, with unusually slow motion, particularly in the cases of several which equalled Jupiter in brightness. One at 10h. 32m. G.M.T. starting from near λ and μ Ursæ Majoris, appeared to receive a sudden check, and was stationary for two seconds 3° below α Canum Venaticorum, where it was nearly extinguished, but a faint portion left a train for several degrees further. It was not easy to judge of the radiant point owing to continual interruption from passing clouds, but it would probably be somewhere about the stars in Ursa Major above-named. Much lightning on this evening. The zodiacal light well seen as far as the principal stars of Aries.

THE MELBOURNE OBSERVATORY.—The Eleventh Report of the Board of Visitors of the Melbourne Observatory, with Mr. Ellery's Annual Report for the year ended 1876, June 20, has been received. In addition to the large reflectors, the Observatory now possesses an 8-inch equatorial, both instruments in excellent working order. With respect to the former, Mr. Ellery remarks that, although at present the mirrors retain their high reflecting polish exceedingly well, it is not to be overlooked that the time must arrive when they will require to be repolished, and in anticipation of this eventuality, which may occur sooner than is now looked for, he intends to devote time during the ensuing year to practice in grinding and polishing large surfaces. Out of about 150 nights during the year to which the report applies, which were more or less fit for observing with the reflector, forty were solely occupied with visitors. The astronomical work accomplished includes the examination, measurement, and sketching of seventy of the nebulæ and clusters of Sir John Herschel's southern work, of which the greater number have been drawn and described in a manner suitable for publication. Mr. Ellery adds:—"The result of these observations indicates that several of the nebulæ are considerably changed, while others appear so completely altered as to be scarcely recognisable, save by their position with respect to adjacent stars. The nebula about η Argus have been compared with a drawing made in March, 1875, but no decided changes were detected. The weather was so far unfavourable at Melbourne for certain classes of observations that out of ninety conjunctions of Saturn's satellites only ten could be observed. No material change in the regular work of the Observatory is contemplated during the year following the conclusion of the report. Observations with the transit-circle would be continued assiduously as in previous years, the Government Astronomer regarding this as the fundamental work of the establishment, which has already given it a reputation in the world, and he quotes in proof of this the opinion expressed by Sir George Airy, that the Melbourne Observatory had produced "the best catalogue of stars of the southern hemisphere ever published." The revision of Sir John Herschel's figured nebulæ will also be continued, with occasional planetary work, as drawings of Mars and Jupiter, observations of conjunctions of Saturn's satellites, &c.

The early publication of results obtained with the great reflector is strongly urged by Mr. Ellery, and all astronomers will concur in his representations upon this point. Difficulties, no doubt, must exist in giving such results to the astronomical world in a perfectly satisfactory manner; nevertheless, Mr. Ellery thinks if a plan he proposes is approved, these difficulties may be surmounted, and all the completed work with the reflector may be forthwith published. We can only express the hope that work of such great interest, and which may so greatly add to the reputation of the Melbourne Observatory, will soon be in the hands of the public. The importance of early publications of astronomical work in these days can hardly be exaggerated.

METEOROLOGICAL NOTES

NEW DAILY WEATHER MAP.—We hail with the greatest satisfaction the appearance, on New Year's Day, of the first number of a daily international weather-map issued by the Austrian Meteorological Institute. It embraces nearly the whole of Europe, and supplies a want not met by any existing weather-maps, in representing the weather of Central and part of Southern Europe, with a satisfactory fulness such as the meteorology of this important region demands in the development of this branch of the science. In addition to the invaluable material this publication will lay before us from day to day relating to thunderstorms, the summer rains, and the falls of hail and snow of Central Europe, it will also furnish data absolutely indispensable in investigating the causes which determine the course and the rate of progress of the storms of North-western Europe. Indeed, in this respect, and consequently in the prognosis of British storms, the Austrian empire is, of all countries which lie eastwards of Great Britain, second in importance only to Lapland and the north of Scandinavia.

LOW TEMPERATURES.—During recent weeks some remarkably low temperatures have been recorded in various countries. During a heavy storm which occurred on December 17 over

all Canada and the north of the United States, and which was attended with considerable damage, the temperature fell at Ottawa to $-30^{\circ}\cdot 0$. What makes this temperature noteworthy is that at the same time the wind continued to blow with great violence, the low temperature being thus not confined to a few feet of the surface, but that of the aërial current passing over Ottawa at the time. On January 4 the temperature fell at Hernösand, in Sweden, and also in Lapland to $-31^{\circ}\cdot 2$. An anticyclone of limited extent, with the characteristic calms and light winds, overspread this region at the time, and it is to be noted that the space of excessively low temperature embraced an area virtually coincident with, and equally as limited as, that of the anticyclone. Still lower temperatures are reported from the interior of Russia. The *Golos* gives the following information as to the unusually low temperatures which prevailed in Northern Russia before Christmas. The thermometer of the Physical Observatory at St. Petersburg (in town) showed on the 22nd, at 9 A.M., $-37^{\circ}\cdot 8$ Cels., and in the Botanical Garden (in the suburbs), between 7 and 9 A.M., the following temperatures were observed:— $-38^{\circ}\cdot 1$ on the 20th, $-39^{\circ}\cdot 4$ on the 21st, and $-41^{\circ}\cdot 9$ on the 22nd ($-43^{\circ}\cdot 4$ Fahr. On the last-named day the mercury was frozen, and the readings were made from a spirit thermometer. So low a temperature as on the 22nd was never observed before at St. Petersburg in December, during the 123 years that regular meteorological observations have been made; and even during the coldest month, January, such low temperatures were observed before only four times, namely, -38° on January 26, 1868; -41° in 1760; $-38^{\circ}\cdot 7$ in 1772; and $-39^{\circ}\cdot 0$ in 1814. The region of low temperatures occupied a very large tract of land, and the cold advanced from the north-east, as was also the case during the unusual cold of 1868. On the 22nd there was observed in the morning, $-40^{\circ}\cdot 4$ at Vologda, $-40^{\circ}\cdot 5$ at Kuopio, in Finland, $-39^{\circ}\cdot 9$ at Bielozersk, -39° in Moscow (-40° in the higher parts of the town), &c. Very low temperatures might have been predicted for some days before, as already on the 20th the cold reached -44° Cels. (-47° Fahr.) in Vologda, and the barometer continued to rise in the whole of Northern Europe, whilst a minimum of pressure traversed the middle parts of Europe and Southern Russia, with comparatively high temperatures and cyclonic winds, which in the north and on the shores of the Baltic blew from the east and the north.

NOTES

A WEALTHY Copenhagen brewer, J. C. Jacobsen, has given the sum of a million of crowns for the promotion of mathematics, natural science, the science of language, history, and philosophy.

As we intimated some time since, the Swedish University of Upsala, founded September 21, 1477, will this year celebrate its 400th anniversary. Great preparations are being made for the event. The University is not only the oldest but the richest in Scandinavia; besides many rich gifts from Gustavus Vasa, it received, among other things, from Gustavus Adolphus, 360 farms, which now yield an annual rent of 200,000 crowns. The funds for maintenance and salaries amounted, in 1870, to 1,758,587 crowns, and the yearly Government grant to 300,000 crowns. The teaching staff consists of thirty-five professors, twenty-seven adjuncts, and fifty docents; the number of matriculated students amounts to about 1,500.

THE Royal Cabinet of Natural History at Stuttgart has just been enriched with an exceedingly rare and valuable palæontological specimen, which is probably without its like in the geological museums of the world. It consists of a group of twenty-four fossil lizards from the sandstone strata of Stuben. The inclosing stone has been with great care entirely removed, showing a strangely intertwined mass, possibly as met by sudden death, but more probably a collection of dead bodies gathered together by the action of the waves. They cover a space of about two square yards, and the individual specimens possess an average length of thirty-two inches. These fossils can be classed with no existing species, but appear rather to possess a combination of diverse characteristics, which at a later stage of development became distinctive features of quite different types. Prominent among the peculiarities are the bones of the extremities, resembling those of existing lizards; the head, which can almost be called a bird's head, and the massive scaly armour, consisting of sixty to seventy successive rings.

WE notice with great pleasure that decided steps are about to be taken to reform the curriculum in Exeter Grammar School It is intended, as soon as arrangements can be completed, that the younger boys shall be taught divinity, English, including history and geography, French, Latin, arithmetic, and the other elements of mathematics, drawing, and some elementary natural science. At a certain point in the school Greek will be added, in accordance with the provisions of the Scheme and the resolution of the Governors; or in lieu of the study of Greek more time will be devoted to mathematics, English, modern languages, and natural science. German will be taught to any boys sufficiently advanced in other subjects to make it desirable. Thus, it is hoped, boys will be adequately prepared for the Universities, for the Public Service, for professional or commercial life. The principle of this new scheme is excellent, and should it be faithfully carried out, Exeter Grammar School ought to become one of the most efficient and complete schools in the country. We hope that the school will receive every encouragement in this laudable effort to provide a complete course of instruction.

THE Vilna Observatory is reported to have been totally destroyed by a fire on December 28. The *Vilensky Vestnik* says that the combined efforts of the town and railway fire brigades, of the troops, and of the students of a college in the neighbourhoood, did not succeed in overcoming the fire and rescuing the great refractor and photo-heliograph. Only books and instruments of smaller value were saved. This is a great loss to science, as the Observatory had done, during the last few years, very valuable work, and some of the beautiful photographs of the sun was exhibited at the South Kensington Loan Collection.

MR. F. B. MEEK, the eminent palæontologist, and for several years a member of the United States Geological and Geographical Survey of the Territories, under Prof. F. V. Hayden, died at Washington, D.C., December 21, aged fifty-nine years. He had just completed the great work of his life, the Cretaceous and Tertiary Invertebrate Fossils of the Upper Missouri Country, in one large quarto volume.

IN the last Session of the Berlin *Anthropologische Gesellschaft*, Prof. Virchow stated that the intrepid young traveller, Herr v. Horn von der Horck, is at present in the camps of the warlike Sioux Indians, busily engaged in obtaining plaster casts for craniological studies. The printed record of v. d. Horck's journey of last summer to the Polar Sea, has just appeared in Germany, and contains much of value written in a very sprightly style. During the first half of the journey zoological and geographical ends were kept in view. On the return trip through Lapland to the Gulf of Bothnia, the expedition assumed an almost exclusively anthropological character. Enormous collections of bones and more especially of skulls were made, and a large number of masks were obtained from the present inhabitants of Lapland. So extensive and complete are these results, that Prof. Virchow regards them as more valuable for the study of Scandinavian craniology than the combined collections of

European museums outside of the Scandinavian countries themselves. The principal geographical result of the journey was the establishment of the fact that a continuous water communication exists between the Polar Sea and the Gulf of Bothnia. On the summit of the watershed between these bodies of water, the lake Wawolo Lampi lays at a height of 800-900 feet above the level of the sea. Two rivers flow from this, one to the north, emptying into the Ivallo, and the other to the south, emptying into the Kititui. Frequent cascades and rapids render this waterway useless for purposes of navigation.

PROF. PALMIERI—the *Times* correspondent at Rome telegraphs on January 7—writing from the Observatory on Mount Vesuvius, says that for the last two days the instruments have shown evident signs of agitation. The smoke from the mountain is issuing with greater force and increased volume. In the interior of the last mouth, opened on December 18, 1865, the fire is no longer visible, in consequence of an immense amount of material having fallen into it, through the giving way of a portion of the crater of 1872. An extraordinary eruptive force will, therefore, be necessary either to make a way through the enormous accumulation of sand and scoriæ or to open some new mouth, whether on the summit or the side of the volcano. In the meantime, the cone is manifest, but it cannnot be stated when it will reach a point sufficient to overcome the resistance.

M. FAYE has been appointed president of the *Bureau des Longitudes* for 1877, and Dr. Janssen vice-president.

A SUBSCRIPTION has been opened at Rouen for the erection of a statue to M. Pouchet, the naturalist, who was the director of the Botanic Gardens of that city, and who died ten years ago. M. Pouchet, as a correspondent of the Academy of Sciences, published many papers in defence of spontaneous generation against M. Pasteur. His works are referred to by Haeckel and Bastian.

AN Admiralty Committee of Inquiry has been appointed in connection with the outbreak of scurvy in the Arctic expedition.

AT the meeting of the Geographical Society on Monday, Mr. Robert Michell read a paper on "The Russian Expedition to the Alai and Pamir." The expedition resulted in much interesting information, which was mentioned in detail, as to the physical features of the country. The president, in winding up the discussion, observed that the regions visited by the expedition and described in the paper were, perhaps, the least known in Central Asia. They contained vast and confused ranges of mountains, some of the peaks of which were among the highest in the world. He trusted that when further expeditions of the kind were organised, steps would be taken by our government to secure that at least two Englishmen of requisite scientific attainment should be allowed to accompany them.

A RECENT thorough survey of the Kasbek-glacier of the Caucasus, has proved that since 1863 it has increased, *i.e.*, its lower extremity has advanced down the va lley, by 826 feet.

THE *Medicinisch-ætiologische Verein* of Berlin decided, in the session of January 4, to call together during the present year an ætiological congress. The following four subjects are announced as the principal topics for the coming conference:—1. Methods of ætiological investigation. 2. Causes of epidemic disease dependent upon mankind. 3. The natural conditions of epidemic diseases. 4. On the *Contagium vivum*.

AT the January session of the Vienna *Zoologisch-botanische Gesellschaft* papers were read by Herr J. Mann "On the Lepidopterous Fauna of the Dolomite Region," and by Prof. Jeitteles "On Treissena Polymorpha."

DR. BREHM, the enterprising Siberian explorer, is at present delivering in the principal German cities, a course of six lectures on the results of his last tour through Northern Asia.

PROF. KLEIN, one of the most promising among the younger German mineralogists, has accepted a call to the professorship of crystallography at the University of Halle.

AUSTRIA follows Germany and other countries in accepting the invitation of the King of Belgium, and an *Afrikanische Gesellschaft* has been organised at Vienna.

THE German Imperial Sanitary Department commences, with the beginning of the present year, the publication of a weekly periodical devoted to sanitary statistics and all subjects connected with the preservation of the public health. Prompt official reports of the mortality in all cities numbering over 150,000 inhabitants, will form a leading feature.

THE Municipal Council of Paris, determined to spare no efforts in order to prevent fresh inundations, have voted the funds for boring a new sewer, or rather a tunnel, which will be utilised for discharging a portion of the Seine, below Paris.

The plan for the rebuilding of the École de Medicine is now ready to be presented to the Municipal Council of Paris. When all the works are completed the total surface covered will be 8,000 square yards; it does not now exceed 3,000. The expense will be 4,300,000 francs.

AN interesting article, by Mr. E. A. Barber, with some curious illustrations, on "The Rock Inscriptions of the Ancient Pueblos of the Colorado, Utah, New Mexico, and Arizona," will be found in the *American Naturalist* for December.

IN an article in the *Révue Scientifique* of January 6, M. H. Le Chatelier shows that there is no geological evidence for the existence of a great inland sea in North Africa, though there was probably in the district of the Tunisian Chotts, at one time, a small isolated salt lake. All the phenomena of the region of the Chotts and of the Sahara may be explained by the action of existing forces, which might at some future time cause a thin layer of salt water to reaccumulate over a small extent of surface.

WITH reference to our note (NATURE, vol. xv. p. 167), on Mr. Allen's work on the North American Bisons, which we stated was issued by the University Press, Cambridge, U.S., we are informed that this "University Press" has no relation with the University. It is simply a name denoting its position *near* the University grounds to distinguish it from another large printing establishment known as the "Riverside Press," also at Cambridge. The memoir noticed formed a part of the "Memoirs" issued by the "Museum of Comparative Zoology," which have taken the place of the former "Illustrated Catalogues," the title having been changed so as to enlarge the scope of the 4to. publications of the old numbers of the Catalogue collected into volumes to form the first volumes of the Memoirs.

ON December 20, the Bremen *Verein für die deutsche Nordpolfahrt*, changed its name to that of *Bremer geographische Gesellschaft*. The question of Polar exploration has assumed such dimensions that a private society cannot hope to accomplish much unaided in this direction. The society will henceforth devote its energies to the solution of geographical problems in other parts of the world. Its occasional communications are also to be replaced by a regular periodical appearing quarterly, under the editorial supervision of Dr. Moriz Lindeman. A special feature of the new society will consist in frequent courses of lectures from the most famous of recent explorers; Brehm, Güssfeldt, and Baron von Schleinitz, are announced as first on the list.

LATE letters from Sydney report the arrival of the Rev. G. Brown, who has been spending the last year in Polynesia, passing from one island to another in the mission brig *John Wesley*. Many interesting discoveries were made in the islands of New Britain and New Ireland. The inhabitants of both islands are

cannibals, but indulge in the custom in order to show their complete mastery over their enemies, and not from a preference for human flesh. New Britain was coasted entirely and crossed several times. The interior is hilly, the loftiest point being 2,500 feet high. It is well populated, and the natives expressed the usual surprise at seeing white men for the first time. The tribe at Blanchi Bay informed the travellers of another tribe at some distance from the coast, who were provided with caudal appendages of an exceedingly remarkable character, and promised to obtain a specimen before the next visit of the brig. At another place, the wealthier families among the natives were accustomed to confine their daughters for several years before the attainment of puberty in tabooed houses, not allowing them to put foot upon the ground during the whole period. A superior tribe was encountered at Spacious Bay, with lighter complexions and straighter hair than their neighbours. Both sexes wore partial clothing. Large collections were brought back illustrating most fully the geology, the fauna, and the rich tropical flora of the islands.

M. Clement Ganneau, who has recently been in London to study the Semitic monuments in the British Museum, writes to the *Times* animadverting on the complete want of system in their arrangement. The Semitic remains are scattered among other collections in such a way as to make their examination a work of the greatest difficulty, whereas were they properly classified and arranged by themselves they would form a Semitic Room without a rival.

The Geographical Society of St. Petersburg has received a telegram from Prjevalsky announcing that he has crossed the Thian Shan, and, on October 14, was fifty versts from Karashar. The country he was then in is a desert.

M. Waddington, French Minister of Public Instruction, is busy fitting up a large pedagogical museum, which will be located in the hotel of the Ministry, and be open to the inspection of any scientific men interested in the progress of pedagogy.

The first portion of the German *Jahresbericht über die Fortschritte der Chemie*, for 1875, containing 480 pages, about one-third of the complete work, has just been issued. General and physical chemistry receives 150 pages, inorganic chemistry, 80 pages, while the remainder of the number is devoted to organic chemistry, which will also occupy the greater portion of the second number. Prof. Fittica, of Marburg, assumes, with the volume for 1875, the chief editorial supervision, and is assisted by the following able corps:—K. Birnbaum, C. Böttinger, C. Hell, H. Klinger, A. Lauberheimer, E. Ludwig, A. Michaelis, A. Naumann, F. Nies, H. Solkowski, Z. H. Skraup, and K. Zöppritz. Complete sets of the *Jahresbericht* are difficult to obtain now as seven years' numbers are out of print. A perfect set from 1847 to the present date, with the two registers, costs from 500 to 600 marks in Germany. The editor requests from the authors of chemical articles separate copies of their communications in order to lighten the labour of classification and compilation.

We have received vol. i. of the *Proceedings* of the Davenport (Iowa, U.S.) Academy of Natural Sciences. This Academy had a very small beginning in 1867, but is now in a flourishing condition. The volume contains the proceedings from 1867 to 1876, and includes some papers of real value, especially on mound exploration. The number of scientific societies in the U.S. issuing publications containing papers of genuine scientific importance is now large, and constantly increasing.

The artificial lighting of rooms affects the human system, on the one hand, through the change produced in the composition of the air by gases of combustion, and on the other through rise of temperature. These influences have lately been examined by M. Erismann (*Zeitschrift für Biologie*). In a part of the laboratory 10 cubic metres' capacity, inclosed by wooden and glass walls, various materials were burnt eight hours, viz., stearine candles (six at a time), rape oil, petroleum, and ordinary gas, and the air was drawn off at different heights and analysed. The results do not pretend to absolute exactness, but a comparison of them is interesting. The tables first show that under all circumstances, and with all sorts of artifical lighting, the air of an inclosed space contains more carbonic acid and organic carbon-containing substances than in absence of such illumination; still, in these experiments the carbonic acid was never greater than 0·6 or 0·7 per 1,000, while the proportion of other carbon compounds was very variable, so that the amount of carbonic acid gives no exact criterion for the vitiation of the air. The CO_2 actually found in the air was only a very small fraction of that produced by the combustion; by far the greatest part must have been carried away by the natural ventilation. In comparing the four materials, the proportion of CO_2 and other carbon compounds was reduced to a light strength of six normal candles. It appeared that the petroleum, with lamp of good construction, communicates to the atmosphere, not only less CO_2, but (what is much more important) fewer products of imperfect combustion than the other lighting materials; and, further, that stearine candles, with the same light-strength, vitiate the air most. As to temperature, that of the lower layers of air, up to a height of 1·5 metres, rose very little during the eight hours, about 2° to 3° on an average, while the upper layers increased considerably in temperature, especially just under the ceiling; this increase, in the case of ordinary gas, rape oil, and petroleum, was 10·5° to 10·8°, in that of candles only 4°. If, however, we take into account the photometric light-effect of the flames during the experiment, it is found that, with equal light-strength, rape oil and gas raise the temperature considerably more than petroleum, and the action of the latter, indeed, came to about that of the candles.

The additions to the Zoological Gardens during the past week include an American Black Bear (*Ursus americanus*) from North America, presented by Mr. W. Stead; a Common Partridge (*Perdix cinerea*), European, presented by Mr. H. Laver; a Razor-bill (*Alca torda*), European, presented by Mr. W. Thompson; two Common Swans (*Cygnus olor*), a Common Cross-bill (*Loxia curvirostra*), European, purchased.

SOCIETIES AND ACADEMIES

London

Zoological Society, January 2.—Prof. Newton, F.R.S., vice-president, in the chair.—Prof. Newton exhibited and made remarks on a specimen of a variety of the guillemot (*Alca troile*) with yellow bill and legs, which had been lately shot by Mr. J. M. Pike on the south coast of England.—Prof. Garrod, F.R.S., read a paper on the osteology and visceral anatomy of the Ruminantia, in which many facts concerning the anatomy of the Cervidæ and the Cavicornia were brought forward, especially with reference to the shape of the liver and the structure of the generative organs in these animals. Among the most important of these was the observation that the uterine mucous membrane of the musk-deer (*Moschus moschiferus*) presents no indications of the presence of cotyledons, the contrary being the case in all other ruminants. Prof. Garrod likewise made a suggestion as to a proposed method of expressing the relations of species by means of formulæ.—A paper by Messrs. Sclater and Salvin was read containing the descriptions of eight new species of South American birds, namely (1), *Euphonia finschi*; (2), *Pheuticus crissalis*; (3), *Octhæca leucometopa*; (4), *Octhæca arenacea*; (5), *Chloronerpes dignus*; (6), *Celeus subflavus*; (7), *Chamæpelia buckleyi*; (8), *Crax erythrognatha*.—Mr. R. Bowdler Sharpe read a paper on some new species of warblers from Madagascar, which had been recently added to the collection in the British Museum, and were proposed to be called *Apalis cerviniventris*, *Bæocerca flaviventris*, and *Dromæocercus brunneus*, the last-named being a new genus, from Madagascar.—A communication was read from Mr. G. S. Brady, containing notes on fresh-water mites which had been obtained from lakes and ponds in England and Ireland.

Royal Microscopical Society, January 3.—Charles Brooke, F.R.S., vice-president, in the chair.—Dr. Wallich read a paper

on the development, reproduction, and surface markings of diatoms, illustrating the subject by drawings.—An interesting paper was read by Mr. Stephenson descriptive of some very curious diffraction experiments, by Prof. Abbe, from which it appears that the use of "defraction gratings" in connection with stops of various kinds placed above the back combination of the objective were competent to produce precisely the same appearances as were observed in certain well-known test objects.—Some mercury globules mounted in balsam were exhibited under the micro-polariscope, by Mr. Stephenson for Mr. Slack, producing some very curious and interesting optical effects.

MANCHESTER

Literary and Philosophical Society, November 28, 1876.—Edward Schunck, F.R.S., vice-president, in the chair.—The Radiometer. Mr. Harry Grimshaw, F.C.S., communicated the following summary of an extract from the "Panorama of Science and Art," published by Nuttall, Fisher, and Dixon, 1813, 2 vols. :—"After alluding to Boerhaave's experiment on the influence of the 'burning glass' on the motion of the 'compass,' the extract goes on to describe a radiometer constructed by Mitchell, which seems to have been constructed as follows :—A thin plate of copper one inch square was attached to one end of a fine 'harpsichord' wire ten inches long. This was balanced on an agate suspension, and the little copper plate was counterpoised by a grain of shot at the other extremity of the wire. As a result of experiments with the instrument, it was found that the influence of the rays of the sun focussed by a concave mirror two feet in diameter, caused a revolution of one-millionth of an inch in a second. The instrument was protected by some sort of glass shade. The same motion was produced in a vacuum."—Note on a manganese ore from New South Wales, and on a specimen of native silver from New Zealand, by M. M. Pattison Muir, F.R.S.E.

December 12, 1876.—E. W. Binney, F.R.S., president, in the chair.—The lowest amounts of atmospheric pressure during the last sixteen years as observed by Thomas Mackereth, F.R.A.S., F.M.S.—On a mineral water from Humphrey Head, near Grangeover-Sands, North Lancashire, by Joseph Barnes and Harry Grimshaw, F.C.S.—On ternary differential equations, by Sir James Cockle, F.R.S., Corresponding Member of the Society.

December 26, 1876.—E. W. Binney, F.R.S., president, in the chair.—Notice of the "Almanacke for XII Yere," printed by Wynkyn de Worde in 1508, by William E. A. Axon, M.R.S.L.—A notice of some organic remains from the Manx schists, by E. W. Binney, F.R.S., president.—On changes in the rates of mortality from different diseases during the twenty years 1854-73, by Joseph Baxendell, F.R.A.S.

PARIS

Academy of Sciences, December 27, 1876.—Vice-Admiral Paris in the chair.—The following papers were read :—On the analysis of pyrogenic gases, by M. Berthelot.—On some derivatives of dialdol, by M. Wurtz.—Note by M. Chevreul on his more recent works. One is a *résumé* of the history of matter from the atomists and Greek Academicians down to Lavoisier. Another relates to experiments meant to show the difference of *absolute* black from *material* black.—On the secular displacements of the orbit of the eighth satellite of Saturn (Japhet), by M. Tisserand.—Researches on the velocity of the wind, made at the observatory of the Roman College, by P. Secchi. He gives a table of observations from 1862 to 1876, with Robinson's anemometer and a meteorograph. The general daily mean for the whole year is 197·5 km. It differs little from month to month, but the horary distribution is very different in the summer and the winter months. The velocity is greatest in March, least in September. But P. Secchi does not take his figures as representing the absolute velocity of wind in the country, as the College is in a low part of the city; observations on Monte Cavo will be better. He adds tables of mean hourly velocity in the different months.—On the project of an irrigation canal from the Rhone, by M. de Lesseps. The canal (schemed by M. Dumont) is estimated to cost 110 million francs; the irrigable surface (in five departments) might produce annually 450,000 tons of hay and support at least 100,000 additional head of large cattle. The scheme would also permit of submersion of the vines. It could be completed in four years.—M. Faye presented the *Annuaire du Bureau des Longitudes* for 1877, and noted improvements in it.—New measurement of the meridian of France, by M. Perrier. The operations now extend, in a continuous system, from the frontier of the Pyrenees to the Department of Loiret; there are thirty-nine stations.—On the absorbent power of wood charcoal for sulphide of carbon, and on the employment of sulphocarbonic charcoal for the destruction of phylloxera, by M. Laureau. M. Kvassery announced that the vines of Hungary are greatly threatened by Phylloxera.—Study on the reduction of a system of forces, of constant amount and direction, acting on determinate points of a solid body, when this body changes its orientation in space, by M. Darboux.—New theorems in higher arithmetic, by M. Lucas.—Enumeration of various theorems on numbers, by M. Proth.—Third note on the theory of the radiometer, by Mr. Crookes.—Researches on the coefficient of capillary flow, by M. Guerout. This coefficient is found to be smaller, in the same series, the more of carbon the substances contain. The author proved this before for monoatomic alcohols and homologous derivatives of benzine; it is here extended to fatty acids and ethers from the same alcohol, and ethers formed by union of the same organic acid with different alcohols of the fatty series. The coefficient for ethers is much higher than that of the alcohols or acids producing them; the introduction of an organic radical into the molecule of an alcohol raises its fluidity considerably. The determination of this coefficient establishes a sort of classification among isomerous bodies.—Practical study on gluten and on its determination in the dry state, by M. Lailler.—Researches on the physiological properties and the mode of elimination of bromhydric ether, by M. Rabuteau. This agent has properties intermediate between those of chloroform, bromoform, and ether.—Formation of the heart in the chicken, by M. Dareste.—On a *Balænoptera borealis* caught at Biarritz in 1874, by M. Fischer. This is the rarest of European species; only five examples have been known.—On a new globular state of quartz entirely crystallised in only one crystallographic direction, by M. Michel Levy. It is probable that the silica of this globular quartz was isolated in the paste before the end of the movement of effusion which produced the fluidity. The example furnishes a new combination of the colloid and crystalline states of silica.—Note on organic powders of the air, by M. Marié-Davy. The meteorological observatory of Montsouris has been charged by the Municipal Council of Paris to make a regular study of the dust of the air, the ground, and the water in various quarters of Paris, commencing with the new year. The author describes some preliminary observations relating to a recent epidemic.—On a maximum of falling stars, already indicated, in the month of December, by M. Chapelas.

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